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A TEST FOR MEASURING SEXUAL EXCITABILITY IN THE FEMALE RAT

JOSEPHINE BALL

From the Psychological Laboratory of the University of California and the Henry Phipps Psychiatric Clinic, Johns Hopkins University

A method of measuring sexual excitability is needed for various studies of sex behavior such as the relation between sexual excitability and general activity, the effect of drugs, climatic conditions, exercise, etc., on sex activity, or the hormonal basis of mating behavior such as that made by Hemmingsen (7). It would also be useful in calibrating the stimulus for quantitative studies of sex behavior in the opposite sex (Anderson (1), Stone and others (13, 14, 15)). The responsiveness of the female rat is important in any test where she is used to measure the sex activity of the male.

The literature relating to the measurement of sex activity in the female rat was reviewed by Stone in 1932 (12). The only paper that has appeared since then, so far as the writer is aware, is that by Hemmingsen (7) who needed a test of heat intensity when he undertook to correlate quantitatively injected sex hormones with the sex activity of castrated female rats. After careful observation of the mating behavior of the normal female rat, he described twelve types of reaction which he arranged to approximate as closely as possible ascending degrees of responsiveness. He gives an excellent critical discussion of the drawbacks of his method of rating, concluding that it was, nevertheless, adequate for his purposes.

The present paper covers first a description and statistical analysis of the method of testing, and second, results of investigations of normal reactions, including a more detailed and extensive study than has been reported heretofore of the exact relations of sexual excitability to vaginal changes as determined by smears, individual differences in excitability and the con-

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sistency of behavior from one cycle to the next in the same individual.

The test has been worked out over a period of years in several laboratories. It was originated and a few preliminary trials made with it in 1927 in the Anatomical Laboratories at the University of California while the writer, at the request of Dr. Herbert M. Evans, was engaged in testing the sex drive of male rats deprived of vitamin E (2). Most of the data were collected in 1928 in the Psychological Laboratory of that University, with rats lent by Dr. Evans. These data were very briefly reported at that time (3). More recently, slight modifications have been made in the test when it was used for calibrating females to test male behavior in the Psychobiological Laboratory of the Phipps Psychiatric Clinic, Johns Hopkins University, and for this and other experimental purposes in the Department of Embryology, Carnegie Institution of Washington.[1]

THE TEST

The test is of the type described by Stone (12) as the frequency of response method. It is not, however, as such tests usually are in the male, a record of the number of discrete acts during a limited mating with a single partner. It is rather an attempt to measure the amount of response given to a series of roughly graded stimuli. A rating scale was devised to denote gradations of response. No attempt was made to equalize the steps on this cale as the data were much too crude and too limited to justify this refinement.

The stimuli used were first, an artificial stimulus administered by the experimenter and second, three types of males which had been found, empirically, to have differential stimulus value.

The artificial stimulus was one that had been discovered accidentally while handling a large number of estrous females. It consists of a series of light, quick clasps by the fingers of the experimenter just in front of the iliac crests, a crude imitation of the palpation by the male in mounting (Stone). This stimulus is much slower and less accurately placed as well as heavier and spread over a larger area than the natural stimulus, but many females respond to it with hopping, crouching and ear vibration (Long and Evans (10), Stone (11)), especially in the more excitable stages. The fact that some animals do not respond to it in this way, although apparently as responsive to male rats as those that do, makes it a questionable test of sexual responsiveness. It has not been used in more recent work. The reasons for dropping it will be discussed more fully in a later section.

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[1] This most recent work has been supported in part by the Committee for Research on Problems of Sex, National Research Council.

This part of the test will be referred to as the "finger test." It was given in a wooden box about 10 by 15 by 18 inches, with shavings on the floor. The animal was lifted gently from her cage. If there was difficulty in removing her, she was put back and tested later so as to reduce preliminary excitement. She was held 10 or 12 inches above the floor of the box and dropped so that she landed on all four feet. Clasps were then administered at the rate of about two per second for five seconds, then as fast as possible for five seconds, after which she was shoved about, picked up and dropped rapidly and clasped in rapid succession for twenty seconds. There was no set sequence to the stimuli in this last twenty seconds. It was merely rough treatment that often elicits sex responses when simple clasping fails to.[2]

As soon as she responded to any of these stimuli with both ear vibration and crouching the finger test was discontinued and her response to male rats was examined. For this second part of the test she was lifted gently and put down slowly on the floor of an observation cage in which a male had been isolated

[2] The author has found that several stimuli which seem to make the animal excited raise the level of sexual excitability. It is not, however, merely a matter of lowering the threshold of excitability to all stimuli. It is well known that rats will not eat in such a condition. Also, the excitement (or attention value?) of a new environment inhibits, while the excitement of having escaped to the floor and been caught usually increases sexual excitability. The latter stimulus as well as rough physical handling is also a stimulus for a short spurt of active running if the animal is put into an activity drum. These examples show that the simple hypothesis of lowered general threshold due to excitement that might be thought of as "emotional" is an inadequate explanation of the facts.

for at least thirty minutes. These cages were of wire mesh, 8 by 8 by 12 inches, and were empty except for shavings on the floor. The female was placed as far from the male as possible and a stop watch was started at the moment they came together.

Three types of males were used. The first was an immature male that would nose the female but would not mount in the manner typical of sexually mature animals. This male was, of course, quite small, being about 40 to 60 days old. Although he did not mount the female in the typical copulatory manner, he often climbed over her from tail to head in the way described by Stone (11). He would also follow her, nosing and licking all parts of her body, especially the genitalia, presenting in this way only those parts of the usual stimulus pattern that are not specifically sexual. Occasionally this male would suddenly copulate during a test with a particularly excitable female. In this event the male was no longer used for testing and the female was isolated immediately and tested again 15 or 20 minutes later with another immature male.

If there was no sexual response to this first male in 60 seconds, she was placed in the same careful manner with a full grown male that was sexually sluggish but not indifferent to new companions. This male would behave in much the same way as the first male but more vigorously and more effectively because of his greater size. Such a male will often mount the most responsive females, but these females were usually rated on the basis of their behavior with the first male only (cf. section on Rating Scale) and so were not placed with this male. Nevertheless, it was difficult to find a male that would not mount the most excitable of the females that did have to be tested with him (i.e., those that had not given any response to the immature male), as the majority of mature males are either too indifferent or too prompt to copulate. Often a mature male that had been left all night with a receptive female one or two days before the test could be used for this part of the test since potency or copulatory drive does not, in the writer's experience, always fully return for several days after a period of satiation.

If there was no response to this male in 60 seconds, she was dropped into a cage with a large, aggressive male, the most potent stimulus. The writer's experience confirms Hemmingsen's statement that females often respond to aggressive males after having shown no heat behavior whatever with less persistent males, and also that males may often be made more aggressive by allowing them to copulate a few times with an excitable female. This third male should be sufficiently excited to mount any rat put into the cage, even a male. Now, it is not always possible to have such a male, especially in the day time. The lowest grade of excitability which is what is measured by this male is, therefore, often not very reliably measured. The writer's main interest lay, however, in the higher grades of responsiveness. These slight excitabilities were, therefore, measured when possible, but they were left out of many of the calculations.

The males used were 15 selected as the most useful of a group of 40 which had been used in a preliminary series. The reason such a large proportion of the original males were discarded is that the average, that is the most "normal" male, is unsatisfactory because he will almost immediately mount the females that hop, dart and arch promptly, and he will not nose, lick and climb about an inactive female for more than a few seconds. He makes a few exploratory advances and sometimes will continue for a minute or more but he cannot be depended on to do so. He more frequently pays no attention to her if she fails to respond promptly. What makes him still more useless as a stimulus is the fact that if she does respond promptly he copulates immediately before she has exhibited enough precopulatory activity to be rated.

The males selected were used in rotation. Each animal could be used about once in three or four days for a period of 4 to perhaps 36 hours. Some males remained quite active for two nights and a day while others were only active enough to be used in emergency in one test period. These sluggish rats were used in the 10 p.m. and 1 a.m. tests, as these hours had been found to be the most favorable for activity in the rat.

Because of this diurnal variation and also because of the effect of the tests themselves any one individual was not the same type at all times. It was necessary to test the males with non-experimental females before each test period, as it was impossible to depend on continuance of the same behavior that had been shown only three hours before. However, the labor of this preliminary testing was considerably diminished by the recognition of certain especially useful individuals after a week's work with them. This familiarity with the individuals made the prediction of their behavior also more accurate than would otherwise have been possible, thus increasing the standardization of the stimulus.

THE RATING SCALE

The responses to the finger test were rated as follows:

0	No response in 30 seconds.
1	Response only in last 20 seconds.
2	Response during the second 5-second period when clasps are administered rapidly.
3	Response in the first 5 seconds during slow clasping.
4	Immediate response upon being dropped into the box before any clasps have been given.

One rating was given for darting or crouching and another, using the same scale, for ear vibration which is a less frequent reaction, apparently indicating a higher degree of excitability.[3] Thus, if an animal crouched during the first 5 seconds, she was given 3 points, but the test was continued until ear vibration appeared, or for 30 seconds if it did not appear sooner. If ear vibration was first shown during the last 20 seconds, one point was added to the 3 assigned for crouching.

Sometimes there was some doubt as to whether crouching, for instance, was the tense, sexual crouch or just standing still. Usually the position is held tensely for an appreciable length of time, if not for some seconds, but occasionally tenseness is so brief as to be questionable. This was given the benefit of the doubt in the last 20 seconds. That is, one point was given for doubtful response, but the response had to be unmistakable for more than one point to be assigned.

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[3] Subsequent experience with Wistar rats has shown that this is not true for all strains. The Wistar rats frequently gave the ear vibration reaction when it was impossible to elicit any other estrous response.

The double rating for ears and crouching or darting made 9 points on the finger test rating scale, running from 0 to 8.

The scale for response to males was not doubled in this way and it had only 5 points.

0 No response to the most vigorous male.
1 Arching when mounted by the aggressive male but no response to the non-mounting males and no other heat behavior.
2 Darting and crouching late, little or only after copulation.
3 Prompt and continued darting and crouching with the second (large, non-mounting) male.
4 Definite sexual response to the immature male in less than 10 seconds.

There was often some difficulty in deciding between 2 and 3, and between 3 and 4. In such cases the decision between 2 and 3 was made by re-testing with the immature male after the female had responded to the second, larger male. She was often more responsive after this stimulus. If she now gave a definite response to the mature non-mounting male, for 2 seconds or more, she was rated 3, but if there was still little or no response to him, she was given only 2 points.

If the response to the immature male on first testing was almost good enough to be rated 4, but it was 2 or 3 seconds late or not very definite in the first 10 seconds and quite vigorous soon afterward, the female was tested with the second male. If she did not hold tense positions continuously for 10 to 15 seconds (some leeway being given to discount the effect of the vigor of the male's approach) she was rated 3. But if she did come up to this requirement she was again tested with the immature male. If she now continued tense with him also for 10 to 15 seconds after first being touched, she was rated 4, while if she relaxed in less than this time, she was given 3 points.

It must be emphasized here that this tense response means continuous darting, arching or crouching without a moment's relaxation. Turning to sniff, poke or otherwise explore the male constitutes relaxation and is not part of the heat behavior pattern, since it may occur during diestrum.

This description of the determination of rating is as complicated as the original decisions about the various details made during an extensive preliminary series of tests. But once they were decided upon and learned there was little difficulty. During the work reported in the present paper, it took on the average about two minutes per rat to give both finger and male tests, assign a rating and record the vaginal smear. The many details of retesting after uncertain responses and unexpected behavior of the male are necessary for any but very crude work. This decision has been reached only after many months of attempting to assign ratings on a simpler basis and finding it absolutely necessary to retest in a systematic manner to obtain results that approach the reliability essential for quantitative work. Hemmingsen's (7) critical discussion of the ratings he used points out sufficiently the kinds of difficulties encountered, so that it seems unnecessary to discuss them further here.

The difficulty in deciding between adjacent points, together with the facts that male behavior is undependable and that the excitability of the female is sometimes increased by contact with males, indicates the degree of subjectivity involved in these ratings, the degree to which the experience and skill of the experimenter is a factor. Nevertheless, there are two facts which indicate a reasonable correspondence of the ratings as given by the experimenter to the actual degree of excitability existing. The first is the high correlation between ratings assigned in two tests given 20 to 40 minutes apart when these ratings were given under circumstances under which it was usually impossible to remember the previous rating (cf. section on Experimental Procedure). The second is the differential response given by males to females rated by this test (cf. section on Subsequent Modification). The male response is measured by the number of copulations in a limited time, an entirely objective score.

EXPERIMENTAL PROCEDURE

Fifty-one rats were observed through two successive heat periods. The study of 25 of these animals was continued through the following two heat periods also, i.e. for four successive cycles. Five more were observed through three periods but the fourth was so abnormal that it was not included in this report (cf. Results, introductory section).

The ages of the experimental animals ranged from eight to sixteen months, 41 of them being in their tenth month. All but one of them had had one or more litters which had been weaned before the experiment began. They were housed in wire mesh cages like those used for observations, three rats in a cage. They were given a constant supply of water and of a dry food which was a slightly modified Steenbock Basic Ration. Lettuce was fed twice a week. The males were in nearby cages where they could no doubt be smelled but not seen or touched by the experimental females.

Smears of all animals were taken every six hours, at approximately 1 and 7 a.m. and p.m. All of those whose smears contained no leucocytes were set aside for testing, three in a cage. These were tested as soon as all smears had been taken and every three hours thereafter until they responded with some sex activity (rated 1 or more). They were then isolated and tested every three hours until they were rated zero in two successive test periods. If these last two tests were made in daylight hours they were tried again at 7 and 10 in the evening when the activity of both males and females tends to rise. If these ratings were also zero the animals were returned to the home cage and smears were again taken every six hours as before.

All animals were tested with active males at the time of smearing when convenient during the late cornified and diestrous stages even after their return to the home cage, but these tests were so dependent on the male behavior that they were very unreliable and were not carried out regularly.

There were many cases of rats that never rated higher than 1 throughout the whole period of the vaginal changes associated with estrus. Since there seemed to be nothing of interest that could be done with such cycles, it was decided that a rat starting this kind of a heat period should be returned to the home cage as soon as it was certain that no further rise of excitability was to occur. Accordingly, after about a dozen such cases had been recorded, they were compared with some 50 cycles that did show the typical rise. It was found that in the latter cases the rating of 1 was not given more than twice (3 hours apart) before the rise began, except in two cases. In these two cases the first acceptance had occurred in the early daylight hours and the higher ratings had not come until the following night (cf. section on Norms, for a discussion of diurnal variations). Therefore, after this, a rat that showed a third successive rating of 1 was returned to the home cage where her smears were taken every 6 hours as before. If, however, that third behavior test had been made in the daytime, another test was made in the evening to make certain that there was no increase at night. She was usually tested at the time of making smears as long as the cornified stage lasted, but these tests, being unavoidably unreliable, were not carried out with the care and regularity that was maintained in the study of the "active" heat periods, i.e., periods in which ratings of 2 or more were assigned.

In cases where the rat never accepted an active male, she was tested regularly every three hours until the smear showed a large number of leucocytes. She was never isolated during such a cycle.

In most cases the behavior test was repeated 20 to 40 minutes after the first test, to obtain a measure of reliability. The factor of subjectivity involved in the ratings, while relatively slight (cf. section on Rating Scale), was eliminated to a still further degree by keeping successive records on separate sheets so they could not be compared with previous ratings. Occasionally an individual could be remembered but such cases were rare, first because the animals were so similar in appearance and, second, because the routine of recording 10 or 15 ratings every three hours continuously, night and day, for a month entailed a monotony that was not at all conducive to remembering any particular number as attached to any particular rat.

It was, unfortunately, not possible to get a second rating by another experimenter for, while the test is easy to administer, once learned, it is not easy for the inexperienced observer to learn and it is unreliable until one has had considerable experience with it, so that no one could reasonably be asked to coöperate in such a study unless he wished to learn the technique for his own purposes.

The sequence of tests was first the finger test, then the tests with males in the order designated in the section describing the test, and finally the vaginal smear was recorded.

An exception to this rule of sequences was made in the case of rats that had not yet shown estrous behavior and that of rats which had been through the active period and were continuing to accept only persistent males. Rats not yet in the active stage were set aside when they showed any heat behavior with the male and retested in the regular manner about one half hour later and regularly thereafter. Only two or three times did a rat show more of the pattern than reluctant arching after one active period had been observed. When this happened, it could always be detected by the response to the active male and the female was then treated as if she had not yet been through an active period.

RESULTS

Typical cycles and variations

Before discussing the behavior of the experimental animals it is necessary to point out a disturbance of the cycles in many of them. Cycles became longer as the experiment progressed, as is shown in table 1, where cycle length is summarized for those animals which were studied for four heat periods, since these are the only individuals for which there are data on more than one total cycle.

The longer cycles in this table are lengthened not by a long diestrous period but by prolongation of the cornified cell stage. The diestrous period in most cases is no more than the normal two or three days. The increasing length of the cornified cell stage has been summarized in table 2 where all estrous periods have been separated into two groups, those of rats which were discarded after the second period and those of rats which were observed through four periods. This separation has been made because the animals selected for the longer study were chosen on the basis of their short cycles.

The progressive increase in the abnormality suggests that it was due to the experimental procedure, the very sensitive rats responding a little even in the first cycle and only the most resistive remaining normal to the end. Dr. Evans suggested that it might be the effect of the frequent taking of smears.

TABLE 1

Increasing length of successive cycles

LENGTH	CYCLE 1	CYCLE 2	CYCLE 3
days			
3	1		
4	19	12	8
5	7	7	5
6	2	6	5
7		2	5
8		2	1
[illegible]			
10			2
Over 18			1
Over 20			1
Average length...	4.35	5.14	6.49+

Following this suggestion, smear taking was reduced to once a day in the cases which were showing cornified cells in the smears longer than usual. This rest from vaginal stimulation seemed to terminate the condition, for diestrous smears usually appeared in a day or two. No controls were run to make certain that they would not have appeared then anyway, but the apparent response was sufficiently frequent to suggest a causal relationship. That smear taking does affect the length of cycle has been shown by Emery and Schwabe (5), who found that taking smears four times a day with a cotton swab produced irregular cycles and frequent persistent cornification in rats which had normal cycles when smears were taken once a day by the pipette method. The smears discussed in the present paper were taken with a spatula, presumably an even more severe stimulus than the cotton swab.

TABLE 2

Increasing length of cornified cell stage in successive cycles

LENGTH	RATS STUDIED, 2 OESTROUS PERIODS		RATS STUDIED, 3 AND 4 OESTROUS PERIODS			
	1st	2nd	1st	2nd	3rd	4th
days						
1	9	2	12	7	5	5
2	6	9	16	14	12	9
3	4	1	1	6	3	6
4	2	2		1	5	1
5						1
6		3		1	1	
7	1				1	2
8						1
9						
10						1
11		1				
12					1	
13						
14						
15		1				
16						
17						
18		1				
19		1				
20					1	
21		1				
Average length.....	2.27	6.05	1.62	2.17	3.55	3.16

That the prolonged cornification was a symptom of disturbed endocrine balance rather than a local response to irritation is indicated by the fact that they were correlated with decreased sexual excitability. A scattergraph showing this relationship is given in figure 8. Figures 1 and 2, however, present, perhaps more clearly, the difference in sexual responsiveness between the typical "normal" heat period and these pathological cycles.

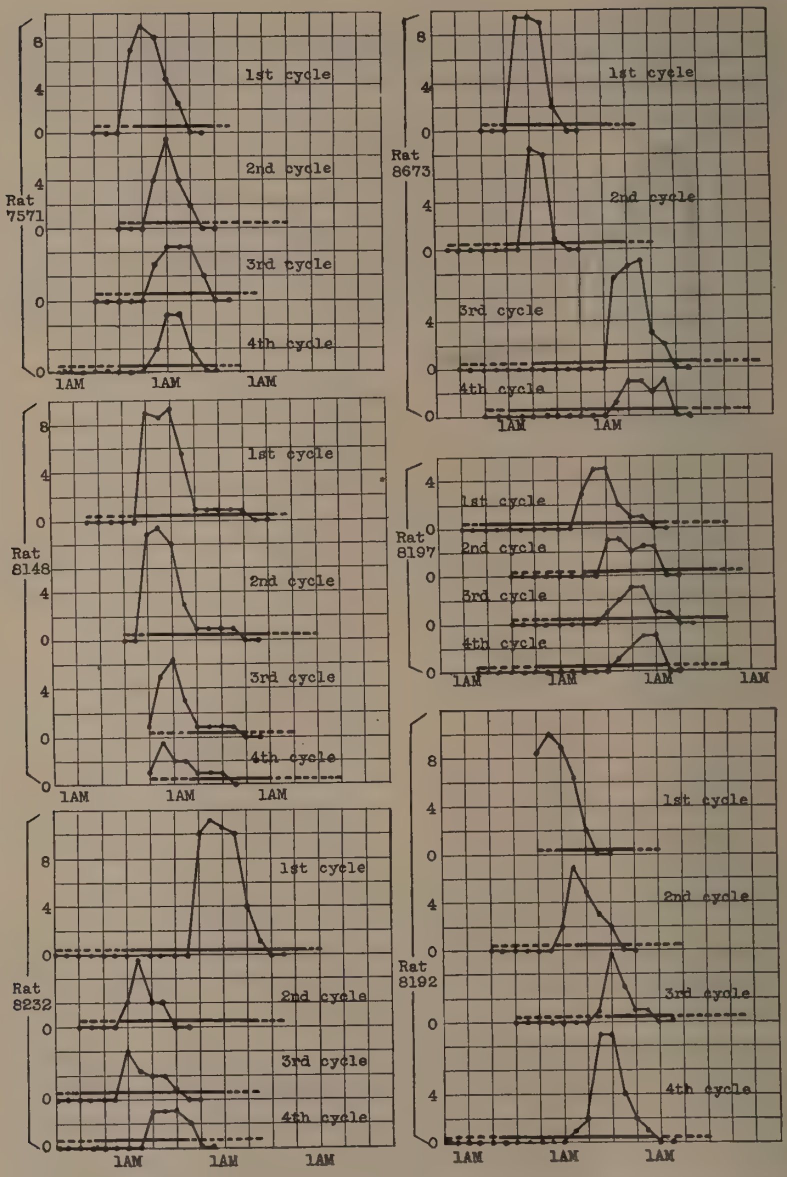

Fig. 1. Samples of Typical Curves Representing the Quantitative Changes in Sexual Excitability during Estrus

Vaginal smears are recorded on the base line, the solid line representing cornified cells, the broken lines representing proestrous and metestrous smears.

Here it will be seen that the prolonged estrous is characterized by the fact that, although the score rarely rises above one, nevertheless the animal may continue in that low state of responsiveness for several days whereas a "normal" curve never maintains a plateau for more than nine hours (i.e. three successive tests). On one occasion during some preliminary tests a rat was found to be in active heat (scores over one) on four successive nights, but this is the only case the author has ever encountered in which a rat was in active heat for more than 24 hours, so that it is as atypical as the continued low excitability.

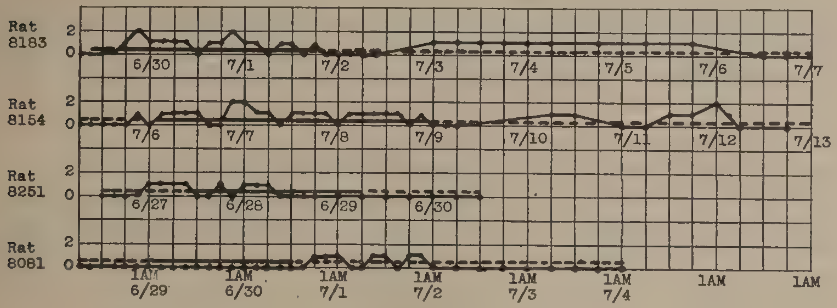

FIG. 2. SAMPLES OF CURVES REPRESENTING THE QUANTITATIVE CHANGES IN SEXUAL EXCITABILITY WHICH CHARACTERIZE CYCLES WITH PROLONGED CORNIFIED SMEARS

———, cornified cells only (Long and Evans' stages 2 and 3).

– – – –, estrous type smears with either nucleated epithelial cells with or without cornified cells as in proestrum or cornified cells with leucocytes and sometimes nucleated epithelial cells as in metestrum.

The entire record for rats 8183 and 8154 is not included. They continued in the same state as represented on the last 4 days of this chart until the end of the experiment, July 23.

Since there seems to be little that one can do with these atypical curves except record and refer to them, and since they skew distributions badly (thus adding to the evidence that they are abnormal), they have been omitted from most calculations.

The typical heat period, samples of which are given in figure 1, began in the early evening, reached its height in the late evening and returned somewhat more gradually to nearly complete loss of sex interest around daybreak. (These tests were made June 22 to July 21.) In the light of the more recent studies in the Phipps and Carnegie Laboratories, the writer feels that probably

TABLE 3

Diurnal variations

HOUR OF OBSERVATION	"ACTIVE" PERIODS STARTING*	PEAK SCORES†	"ACTIVE" PERIODS ENDING*
1 p.m.	4	5	6
4 p.m.	14	6	6
7 p.m.	36	8	1
10 p.m.	23	40	2
1 a.m.	14	34	15
4 a.m.	5	17	23
7 a.m.	7	7	28
10 a.m.	1	6	16

* "Active" period means that part of a heat period in which scores are 2 or more.

† If there was more than one highest score, all are included as peak scores.

TABLE 4

Length of the "active" period in which scores are two or more

LENGTH	NUMBER OF PERIODS
hours	
3	5
6	6
9	21
12	26
15	22
18	12
21	2

TABLE 5

Time of the peak score with relation to the beginning of the "active" period

NUMBER OF HOURS THAT THE HIGHEST SCORE IS REACHED AFTER THE FIRST RATING OF 2 OR MORE	NUMBER OF CASES
0	27
3	60
6	34
9	2

only the first cycles in figure 1 may be considered quite normal. The progressive decrease in response to the test (cf. also table 6) as well as the progressively greater tendency to morning and daytime activity which are shown in these curves has not occurred in later work when smears have been taken less often, so that these changes were probably the result of endocrine disturbance and not due to the fact that the animals had been repeatedly subjected to the behavior test.

The variations from the typical picture are shown in tables 3, 4 and 5 which include all cycles in which there were ratings of 2 or over, even though they may not have been always quite "normal."

The relative smoothness of these typical curves should be noted as an indication that the categories of this rating scale represent progressive steps, a desideratum which Hemmingsen (7) found was not always true of his arrangement of types of behavior (cf. also section on Subsequent Modification).

Relations between responses to the male and to the finger test

Although the finger test has been discarded in more recent work, it seems advisable to retain these scores in discussing these earlier studies because the rating scale for the test with males at this time had only five points, which the finger test scores spread to thirteen. The distributions made by this wider spread seem better for statistical handling even though they are often bimodal and usually skewed. The bimodality is due to the fact that some rats reacted to the finger test and some did not, even though their response to the male might be quite high.

The relationship between the two tests is shown in figure 3 which deals with single tests, and in figure 4 in which the total male ratings for a heat period are plotted against the total finger test ratings for that period. From these charts it will be seen that, while there is some relationship, nevertheless it is not true that the finger test merely measures higher degrees of exactly what the male test measures. For this reason it has been discarded in later work (cf. section on "Subsequent Modification"). It is, however, a useful way of picking quickly some animals that are in active heat. From figure 3 it will be seen that most of those making vigorous response to slow clasping (rating of 3 or more) will be quite active in a mating test. It is, therefore, very useful in rough or non-quantitative work.

Several methods of combining the two ratings into a single excitability score were tried. Doubling the male test ratings tended to equalize the weighting since the range of the finger test ratings is 9 points as against 5 for the test with males. It also, of course, increased the spread and reduced the bimodality of the distributions. But there was little logical reason for equalizing the weighting and the finger test was slightly more

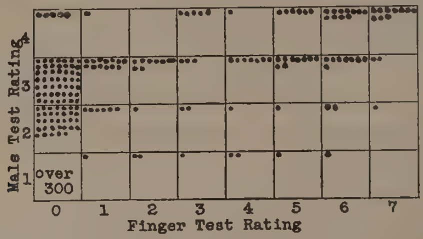

Fig. 3. Scattergraph Showing the Relation of the Finger Test Rating to the Male Test Rating in Single Tests

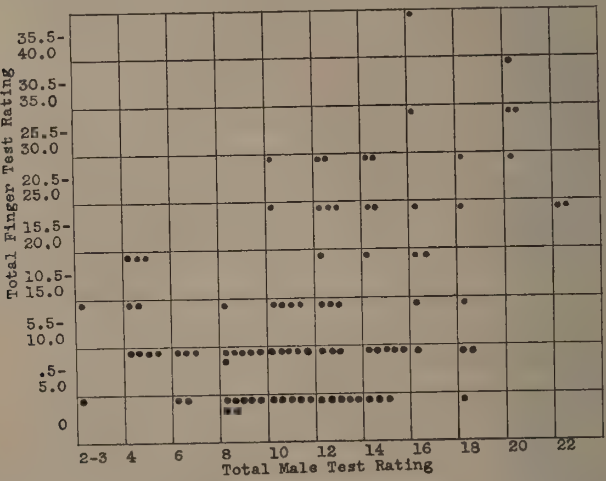

Fig. 4. Scattergraph Showing the Relation of the Finger Test Rating to the Male Test Rating when Total Scores for Entire Heat Periods are Considered

Those rats which never responded to the finger test throughout the heat period have been separated from the rest in a special category because of the possibility that these animals might be different qualitatively rather than quantitatively. Such a separation was considered useful, also to those who might wish to use the finger test as a practical method of picking estrous females.

reliable, its reliability coefficient being .83 as against .79 for the test with males. Moreover, in dealing with total ratings for entire heat periods, the relative scores were almost identical whether the male rating was doubled or not ($r = .99$). The simplest way was therefore adopted and throughout the rest of the discussion the excitability score will be understood to be the sum of the ratings on the finger test and the test with males. There is a possible range of thirteen points on such a scale for any single test.[3]

Reliability

In the section describing the experimental procedure it was stated that a second test was usually given in order to measure reliability. The factor of the experimenter's remembering the rating assigned 30 minutes previously is discussed in that section together with the precautions adopted to eliminate it as much as possible.

Inasmuch as sexual excitability is constantly changing throughout most of a heat period which is only a few hours long, it is not to be expected that two tests made 20 to 40 minutes apart will be the same. If the test is reliable they will be different because the excitability is different, and they will differ most at those parts of the curve where excitability is changing most rapidly. Changes are usually most rapid during the ascent, the descent of the curve being somewhat more gradual. But at the peak where the direction is changing, there is a place where the rate of increase declines to zero and changes to decrease. This is the place where the two tests should be most similar and this is the only point where correlation of two tests 20 to 40 minutes apart will approach a true reliability coefficient. Actually this exact point was only approximated, of course, but using the highest scores for each heat period (discarding periods in which no score was over 1) approaches this ideal.

The scattergraphs in figures 5a, 5b and 5c, show that the actual scores behave in the manner described, those of the ascending limb of the curve showing the least relationship, those at the peak the highest. The fact that these charts do show mostly increase of the second test over the first during the ascent and mostly decrease during the descent of the curve is further indication that the memory of the experimenter was a negligible factor in the measure of reliability, inasmuch as during the time of collection of these data it was assumed that a reliable test would give the same results on the second test at all times. It was only when the results were plotted, all parts of the curve on one scattergraph, and found to show a disappointingly low relationship that the reason for this was suspected. Even then, until the charts were made, it seemed unlikely that 20 to 40 minutes could account for the changes. However, comparison of these three charts shows unmistakably that even this short lapse of time is an important factor.

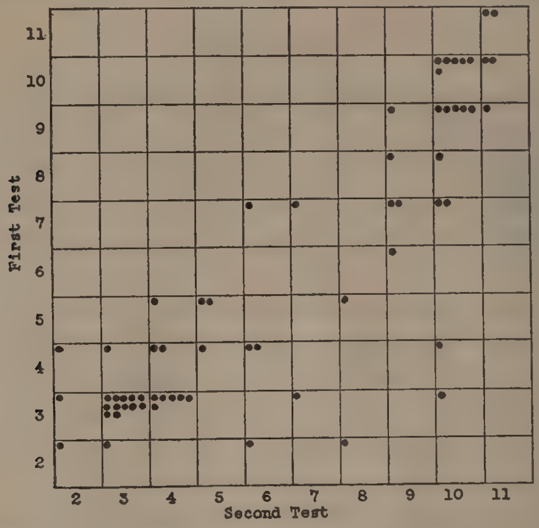

Fig. 5a. Relation of Each Test to a Second Test 20 to 40 Minutes Later

Only scores before the peak score of each heat period are used in this chart. Most of the displacement is to the right, showing that the second test, when different from the first, is usually higher.

When only the highest scores of each heat period were used, the reliability coefficient was found to be .88 ± .02. Brown's formula raises it to .94 for two tests. This may, then, be considered to be the reliability coefficient for the average of the two

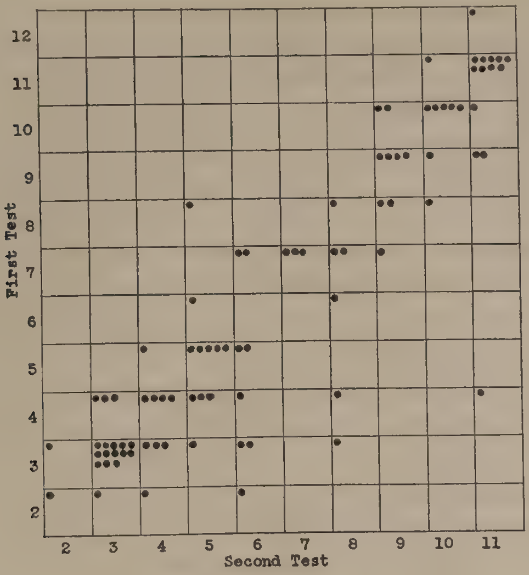

FIG. 5B. RELIABILITY

Only the peak scores are used in this scattergraph. Cf. text for discussion.

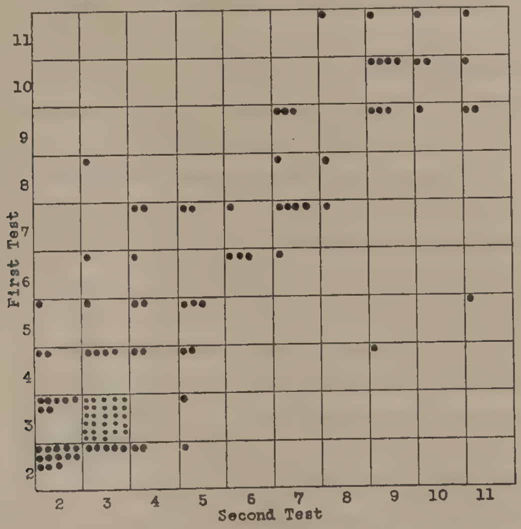

FIG. 5C. RELATION OF EACH TEST TO A SECOND TEST 20 TO 40 MINUTES LATER

Only scores occurring after the peak has been reached are used in this chart. Most of the displacement is to the left, showing that the second test, when different from the first, is usually lower.

tests which is the score used in all subsequent discussion. The bimodality of the distributions raises this correlation above what it would be if they were normal. On the other hand, the fact that the trait measured is probably actually different in many cases tends to lower it below what a true reliability coefficient would be. The scattergraph (fig. 5b) will give the reader a practical, graphic picture of the test's reliability and permit him to see the extent of the bimodality. It shows that, of the 87 cases, 45 are the same in both tests and only 15 change more than one point. Considering that the excitability was probably actually different in many of these cases, it would seem that the true reliability of the test is probably quite high.

Reliability may also be considered from another standpoint, namely the extent to which it measures the sexual constitution of the animal, the extent to which she is a relatively active or indifferent animal as compared to other members of the population. In this sense the test would seem, in this series, to be entirely unreliable, the correlation coefficient for the total rating of first with second heat periods being only .47 ± .08.

To be sure, the pathology of these rats explains, in part, this low correlation. That some constant factor depressed excitability increasingly in successive cycles is shown by the decreasing averages in table 6 which gives total ratings for successive cycles. The curves in figure 1 also illustrate this fact. Nevertheless, different rats were affected to markedly different degrees, 7 of the 51 cases even increasing the score in the second heat period, while 11 of the 30 cases with a third period were better in the third than in the second, and 5 of 25 animals increased their scores in the fourth period. This shows that the pathology, which was increasing, was not the only factor affecting the change of excitability from one cycle to the next.

The variability from one cycle to the next was far greater among these rats than it has seemed in later work with 6 rats in the Phipps Laboratory and 10 animals of Wistar stock, in which smears were taken much less frequently and there was no evident pathology. It is difficult, however, to compare these data in any brief numerical manner because the groups

TABLE 6

*Total score for each rat in successive heat periods**

RAT NUMBER	1ST CYCLE	2ND CYCLE	3RD CYCLE	4TH CYCLE
8102	1	0		
8303	29	14	4	
8311	43	2	45	34
8312	20	19	1	1
8113	13	1	1	10
8014	8	1		
8114	53	16	1	1
8314	13	1	2	1
8315	15	16	8	11
8018	1	2		
8320	12	15	0	1
8121	55	1		
1031	41	47	44	36
8131	1	1		
8132	11	0	6	1
8232	46	12	11	11
8138	37	1		
8638	18	1	10	8
8039	7	8	1	
9039	2	0	7	2
8244	20	9	8	12
8645	12	0		
8148	33	30	15	8
8157	22	2	18	
8151	18	11	0	13
8251	13	1	1	1
8154	28	4		
5061	14	1		
8263	10	15	1	
8264	49	0		
8165	28	37	1	1
8267	2	2		
8570	1	1		
7571	31	18	19	14
8673	30	17	30	11
8276	14	10		
8077	1	1		
8279	1	1		
8281	34	1	17	1
8083	24	25	21	15

* Cases in which no rating was over 1 are recorded as 1, no matter how long they stayed in this state. This is done to distinguish such cycles from more normal heat periods (cf. text, section on Adequate Partial Test).

TABLE 6—*Concluded*

RAT NUMBER	1ST CYCLE	2ND CYCLE	3RD CYCLE	4TH CYCLE
8183	17	4		
8186	7	0		
8091	34	7	28	0
8291	13	1		
8192	36	19	9	26
6492	1	5		
8095	11	1	14	1
8295	12	4	0	
6496	19	12		
8197	15	13	8	8
8199	21	8		
Average......	19.7	8.2	1.1	0.9

are small and no observations were made during the early morning hours. The total curves will be presented in later papers, as they are parts of other experimental studies.

Taking into consideration these results together with similar findings in subsequent work with monkeys, the writer feels that the concept of sexual constitution is very elusive in the present state of our ignorance. Sexual excitability is evidently a phenomenon that is exceedingly sensitive to influence by a great many factors, the majority of which we have not determined, much less controlled. Sexual constitution or the animal's usual or optimal or "native" (i.e., presumably uninfluenced) responsiveness is a trait we are not in a position, at present, to measure with any accuracy. Only after several cycles have been observed can we feel that we know something about whether an animal is a "normally" active or an inactive individual with respect to sex response.

Validity

The problem of validity does not arise, since this is not a sample of an activity similar to what we wish to study, but a sample of the actual trait we are interested in, namely, sexual excitability or the ease with which sex responses can be elicited. Any other form of sex performance involves other traits such as fatiguability, internal changes that affect the stimulus value of the partner after copulation, sexual capacity, and the like, so that even the relation of this test to a longer test of sex behavior is not a measure of its validity as a test for sexual excitability.

Adequate partial test

Since it is obviously impracticable to measure the whole heat period at three-hour intervals, the question arises as to what is the easiest way to get an adequate picture of the total period.

The curve of the activity of a heat period has height, length and area. How are these related? Which do we mean when we think of the rat's "sexual excitability?" Obviously excitability, precisely defined, means responsiveness at a given moment under given conditions. Nevertheless, there is also interest in how excitable she can become, what height of excitability she reaches in any given heat period and how long she holds these states.

"Total rating," which has been used in a number of the above discussions, is a measure of the area, taking into consideration the height she reaches, the length of time she stays high and the length of time she shows any heat behavior. For a "typical" heat period this seems a satisfactory single statement of the total picture for that period. The pathological picture, however, where a rating of 1 is maintained for several days may give a score of perhaps 30 by this method. Such a score is obviously quite incomparable to scores of 30 obtained within 12 to 15 hours such as are shown in figure 1, rat 8673, first and third cycles.

This illustrates the limitations of the use of any one score for all purposes. There are cases where it is necessary to describe the shape of the curve and define its height and length independently. However, this does not mean that the area score, "total rating," is not a satisfactory score for many comparisons. Indeed, it seems, for most purposes, the most satisfactory single statement of the excitability of a heat period.

Accepting it as such, there next arises the problem of how to get a reasonably adequate measure of this without working 24 hours a day.

The length of an active heat period is related to the total rating as shown in figure 6 where it will be seen that an active stage lasting 18 hours is associated with total ratings from 15 to 50, the entire range of possible total ratings. This makes it clear that length, although a dimension of this area score, is by no means the main determining factor and is therefore an inadequate measure of the area score.

The peak score also is a rather unsatisfactory measure since it fails to differentiate the higher total ratings, no doubt because of the inadequacy of the test in measuring the high degrees of excitability. This is shown in figure 7 which suggests an inadequacy of the single tests at the upper end of the scale. Several of the typical curves such as are given in figure 1 show a plateau suggesting that excitability may have risen during the plateau but the test had no higher categories. Nevertheless the peak score is a better measure of the total score ($r = .92$) than is the length of the active period ($r = .72$), and may well be considered adequate for certain types of investigations. The sum of the two highest scores has been used for certain computations in the present paper. It is correlated with the total score slightly more highly than is the peak score alone ($r = .94$).

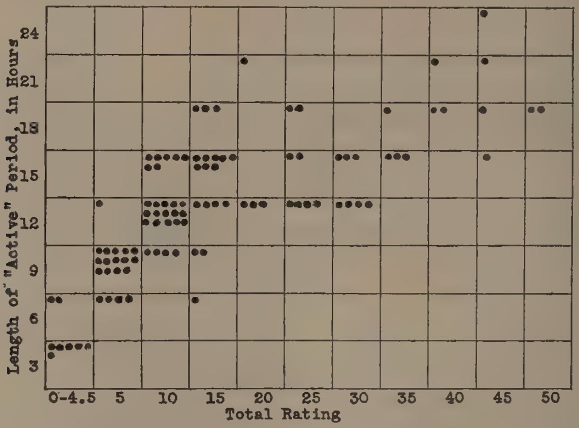

Fig. 6. Scattergraph Showing That the Length of the "Active Period" (Scores over 1) is not a Very Telling Measure of the Total Score for a Heat Period

In order to determine the highest score it is necessary to make observations until the peak has been reached and one more test after that to make certain that the curve has started down. This would save much of the especially trying early morning work, since the peak is reached by 1 a.m. in the majority of cases (table 3). Indeed, for many problems it would be sufficient to make observations only until 1 a.m.

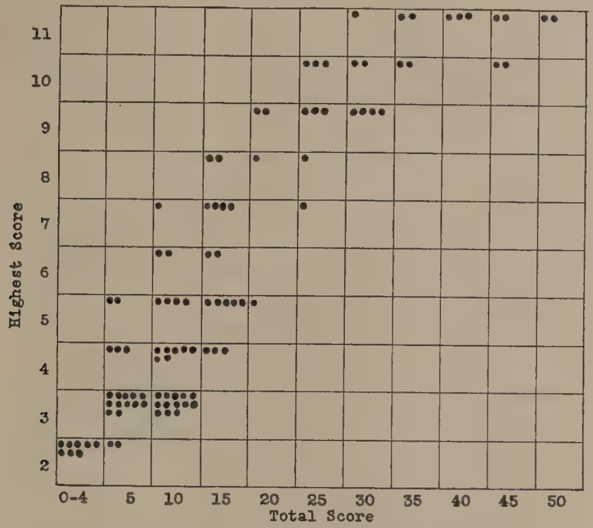

Fig. 7. Scattergraph Showing How Far the Highest Score in a Heat Period Can be Depended on to Predict the Total Score for the Entire Period

It is quite possible that later work may improve the upper end of the scale for single tests in which case the peak alone may be an adequate statement of total excitability. The modifications described at the end of this paper would seem to be a step in this direction.

Effect of the endocrine disturbance on sexual excitability

In the discussion of the abnormally long cycles presented above, it was stated that there was a relation between the length of the cornified cell stage of the smears and the sex behavior. Being more familiar with the meaning of the test scores, the reader is now in a better position to evaluate the statistical basis for that statement.

Since the "total rating" scores do not properly characterize the type of excitability curves in which scores are very low but the period of acceptance continues for a day or more, the two highest ratings have been used in figure 8 in which this score has been plotted against the length of the cornified cell stage for each period. The numerical expression of this relationship gives less information than the mere inspection of the scattergraph. Such inspection will show that there is little relationship between the two factors when the cornified cell stage is less than three days long, but that at this point excitability is seldom high and when it is more than three days long there is not often more than mere acceptance. Evidently the same factor that caused the slow return to the diestrous condition of the vagina also depressed sexual responsiveness. The same

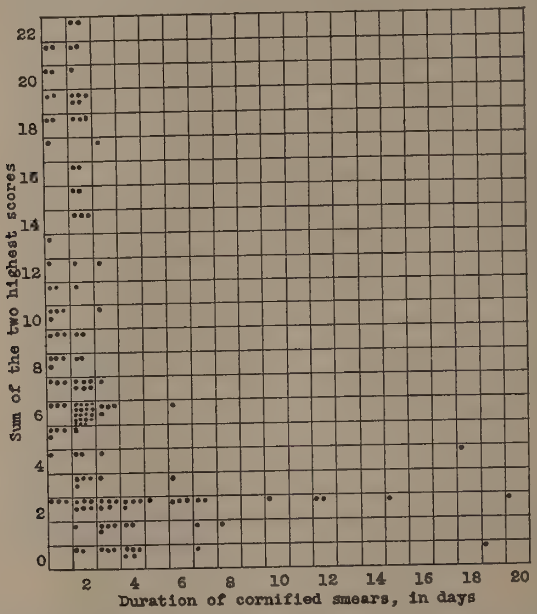

Fig. 8. Scattergraph Showing That Sexual Excitability is Below Normal in Cycles in Which There is Prolonged Vaginal Cornification

chart shows also, however, that excitability may remain at this low level even when vaginal changes appear normal.

Norms

The data were studied with a view to determining norms as well as testing the test, but the pathological cycles have made them less valuable for this purpose than had been expected. Furthermore, subsequent experience with a different (Wistar) strain of rats has suggested that norms will have to be established for each colony for any detailed comparisons. However, in spite of the pathology and the slight differences that will be found in other strains, actual performance is, neverthless, of some interest.

Probably the type of data given above in the section on Typical Cycles is the most dependable. The tables accompanying that discussion, besides repeating what is well known, that most of the sex activity of the rat occurs at night, also show definitely, first, that the most economical time to work on problems requiring very active females is 9 p.m. to 2 a.m. (tables 3 and 7) and second, that once a rat has passed the stage of mere acceptance shown by a rating of 1, her highest activity will probably occur within the next six hours (table 5).

The same tables also show that there was no hour of the day or night of which it is possible to say that there were no receptive females. They may start or stop or reach their peak at any hour in spite of the definite tendency toward the well known usual hours. It is possible (cf. section on Typical Cycles) that the endocrine disturbance increased the daytime activity. However, it is appropriate to state that the work was done in a poorly lighted room in which there was never any sunlight and in which artificial light sufficient to read by was kept burning constantly, although it was not needed in the daytime. In view of the observation by Dempsey, Myers, Young and Jennison (4) that the diurnal rhythm in the hour at which guinea pigs come into heat is obliterated when the animals are kept in a dark room, it might be thought that the nocturnal tendency of these rats was somewhat less clear than it would have been had they been kept in a room with more daylight. However, the rats used for similar tests at the Phipps Clinic were kept in a dark room continuously throughout the experiment as well as for some weeks before it, and the diurnal rhythm was quite as definite as in the present series. From these data, then, it would seem that the changes in light are not the cause of the nocturnal sex activity of the rat and that, in rats kept in a room well lighted by daylight there would also be variants from the general tendency to nocturnal estrus. But Hemmingsen and Krarup have reported that they have been able to reverse the diurnal rhythm of rats by reversing the light rhythm (8). Thus, the question of norms in diurnal variation is still in doubt.

TABLE 7

Diurnal distribution of scores

HOUR	RATINGS ASSIGNED*									
	2	3	4	5	6	7	8	9	10	11
1 p.m.	5	4	2	2	1	0	0	1	0	0
4 p.m.	9	7	3	1	0	1	1	0	0	0
7 p.m.	11	20	3	0	3	2	4	6	4	0
10 p.m.	13	20	8	7	1	2	4	7	8	6
1 a.m.	18	23	10	7	7	6	3	2	6	4
4 a.m.	18	25	6	5	3	5	0	4	3	1
7 a.m.	34	11	3	2	0	2	2	3	0	0
10 a.m.	16	3	1	2	2	0	1	3	0	0

* To eliminate obviously pathological cycles, ratings under 2 are not included (cf. text).

There are two other facts of this type not yet discussed. The first, the number of rats at any stage of excitability at each hour of the day, would seem to be less influenced by the disturbed endocrine system than the second, the relation of stages of excitability to the vaginal smear.

The first relationship is shown in table 7 where it will be seen that while there are rats in heat at all hours of the day, it is seldom that they are very active in the hours that are usually most convenient to work, and again, that the most active hours are those around midnight. In reading this table it must be remembered that the three cases of the rating 9 assigned at 10 a.m. means that only 3 times in 30 days was it possible to find such excitable animals at 10 a.m. in a colony of 51 females, and the grouping of the cases in the rest of the table suggests that one should expect even fewer if the experiment were to be repeated (cf. also section on Typical Cycles).

The temporal relation of behavior to vaginal smears can be best understood by inspection of figures 1 and 2 where smears are indicated on the base line. Table 8 shows the relation of time of the beginning of "active" periods (i.e., first rating over 1) to vaginal smears. These data are given because of the surprising variation from the "normal" as described by Long and Evans (10). Possibly this variability is partly due to the disturbed endocrine condition of most of the group, but it can be seen in figure 1 that a number of cycles show no other variation from normal than the lateness of the beginning of heat behavior as compared to smear changes. Even the first cycles of rats 8197 and 8232, as well as the third cycle of rat 8673 and the fourth cycle of rat 8192, for instance, have heat periods of which the curve of excitability is fairly typical as to height, spread and shape, and the cornified cell smears do not last

TABLE 8

Time of the first score over 1 in each estrus in relation to vaginal smears

NUMBER OF HOURS FROM THE BEGINNING OF LONG AND EVANS' STAGE TWO	NUMBER OF "ACTIVE" PERIODS STARTING
9 hours before	1
6 hours before	4
3 hours before	5
0	11
3 hours after	5
6 hours after	5
9 hours after	4
12 hours after	4
15 hours after	1
18 hours after	1
21 hours after	1
24 hours after	0
27 hours after	1

more than two or three days and yet these animals refused to mate until 18 to 24 hours after all nucleated cells had disappeared from the smears. There are also cases shown in figure 8 where smears were normal and there was no heat behavior. Furthermore, similar discrepancies between smears and behavior have been described by Hemmingsen in the rat (7), by Grant in the ewe (6) and by Young, Myers, Dempsey and Jennison in the guinea pig (17). It would appear, therefore, that the disturbed endocrine condition does not entirely account for the irregularity observed in this group of rats.

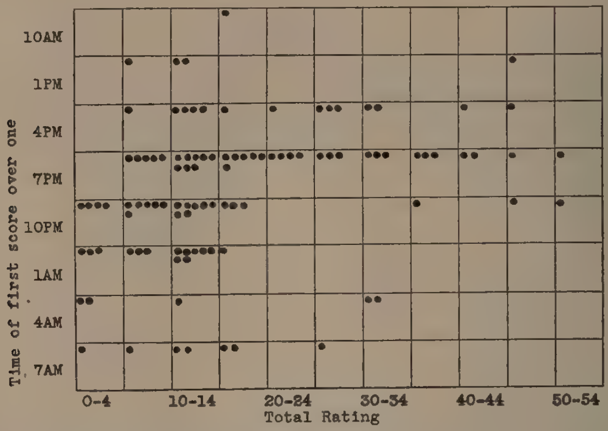

Fig. 9. Scattergraph Showing That Most of the Especially Active Heat Periods Began in the Afternoon or Early Evening but the Relationship is not High

There is some indication that excitability waits for night hours regardless of when vaginal changes start, but this is not always true. In fact, there are cases in which the first cornified cells appear in the evening when estrous usually starts and yet the heat behavior is delayed until morning, or even the following morning, as in the third cycle of rat 8673, making one of the unusual cases of daytime estrous.

Often these daylight heat periods are less active than those occurring at night but this also is not always true. The third and fourth cycles of rat 8673 (fig. 1) illustrate both high and low daytime excitability in the same animal. But figure 9 shows that there is a real tendency toward lower excitability if the "active" period starts in the morning. In this chart the total rating for each period is plotted against the hour at which it started.

The preceding paragraphs indicate that there is some relation between excitability, time of day and type of smear, but there seems to be no way of showing this statistically with so few observations, since it is necessary to hold constant the factors of pathology and individual differences between rats before these other three variables can be studied.

Individual differences in the excitability of these animals can hardly be shown since there was such wide variability from one cycle to the next that it is evident that the tests gave no reliable indication of an animal's "normal" excitability (cf. section on Reliability for further discussion of this concept).

The variable relation of sexual excitability to vaginal smears suggests one difficulty with the obstruction box technique as it was used by Jenkins (9) to study sex drive. For instance, this author describes the behavior of two females which she assumes are in heat on the basis of the smears. One of these animals does a great deal of kicking and fighting when the male attempts to mount although she occasionally permits copulation. Jenkins interprets this behavior as the result of pain during copulation, but actually she has described the typical picture of a female just entering the heat period. Probably if she had tested this animal two or three hours later, copulation would not have looked any more painful than it did in the case of the second female she describes. Such a partially refusing female might well have had the cornified cell smear Jenkins required as a criterion of heat.

This is just one example of what probably happened frequently in all the female groups in which animals were picked on the basis of general behavior and smears. Estrus is not an all-or-none phenomenon and cornified cell smears do not guarantee equal degrees of sexual excitability nor, indeed, any heat behavior at all. As for general cage behavior in the absence of males, both Jenkins and Warner (16) found that this was not an infallible criterion of heat, since they sometimes picked, on this basis, animals with diestrous smears. Therefore, groups of 6 to 21 such as those used in the studies by Jenkins might easily have differed from each other, not because of the experimental difference in treatment, but merely because one group might, by chance, have contained mostly females of low excitability, which would have been higher a few hours earlier or later in that heat period, while another group might contain mostly females at the peak of that cycle's activity. Critical ratios for the groups where the females were the experimental animals may have controlled for this to some extent, although, so far as the writer is aware, the extent to which the obstruction box measures sexual excitability in the female has not yet been determined. Critical ratios, of course, do nothing to assure us that differences found between male groups were due to the intended experimental difference in treatment and not to the difference in stimulus value of the incentive females.

SUBSEQUENT MODIFICATION OF THE TEST

The discussion in the section dealing with the relation between the finger test and response to males will show why, in later work, the finger test has been dropped and the male test rating spread by making finer distinctions between various amounts of reaction to males. The scale now in use is as follows:

0 No response to the most vigorous male.
1 Slow ("reluctant") arching and nothing else, to the most vigorous male, or arching promptly sometimes but refusing at least half of his approaches.
2 Prompt arching when mounted but no other heat behavior.
3 No response to the second (non-mounting) male, but more than arching to the third (aggressive) male.
4 No response to the immature male and very little to the second.
5 No response to the immature male; fairly active with the second.
6 Some brief response that is definitely sexual to the immature male but it is not continued.

7 Repeated brief periods of tenseness in response to the immature male but no period lasts for 5 consecutive seconds.

8 One or more periods of tenseness lasting 5 to 10 consecutive seconds.

9 Such a period of tenseness lasting 10 to 15 seconds.

10 Such a period of tenseness lasting 15 to 20 seconds.

11 Such a period of tenseness lasting 20 to 30 seconds.

12 Such a period of tenseness lasting over 30 seconds.

Since 7 of these 13 ratings are made on the basis of the response to the immature male it becomes important to make this stimulus as constant as possible. The male should weigh about 75 to 150 grams and he should be very active in his attention to the female. If one immature male is not very active she may be tried with others in which case her best response is the one to be used in rating her excitability. Brief stimulation by these small males seems to have little effect on her subsequent behavior.

This rating scale has not been tested as systematically as the older one but there is one very objective criterion of its value. It has been used to calibrate females for use as stimulus animals to test the sexual excitability of males. In this experiment females rating 5 on this scale were desired but, as it was not always possible to find a female at exactly this stage at the moment when the males were to be tested, females rating 4 and 6 were often used. Twenty tests were made with rats rated 4, 106 with animals rated 5, and 84 with animals rated 6. The tests were two minute tests made of 6 males which were tested every night for 5 weeks.

Under these circumstances the males failed to copulate in a larger proportion of the tests with females with the lower ratings. Copulation occurred in 63 per cent of the tests with females rated 6, in 55 per cent of those with females rated 5, and in only 40 per cent of the tests with females rated 4. This regular progression of response to degrees of excitability rated in adjacent categories is offered as testimony to the accuracy of calibration. It is also further evidence (cf. section on Typical Cycles) that the categories in this type of rating scale are progressive steps.

This scale is presented as an improvement on the old one because, while resting on the same principles, it is changed in the ways indicated by the data on the old one, i.e. it discards the finger test and overcomes the objection that the response-to-male scale has too few categories by tripling the number of these categories, spreading them especially at the upper end, since there was some evidence of plateaus at the peaks of heat periods as if the old scale had failed to differentiate the highest degrees of excitability (cf. figures 1 and 7, and discussion of "Adequate partial test"). The above indication of its value in calibrating stimulus females for work with males would seem to justify its presentation despite the lack of systematic statistical study.

SUMMARY

A method of testing and rating the sexual excitability of female rats is described.

A reliability coefficient of .94 was obtained for the test.

Curves of typical heat periods are presented together with tables showing diurnal distributions of beginning, peak and end of estrous periods and of the time of occurrence of the various degrees of excitability.

Detailed time relations between vaginal smears and behavior are presented.

Adequate sampling of the total heat period is discussed.

A pathological condition is described as being characterized by prolonged cornification of the vaginal mucosa and by long continued, very low sexual excitability. The condition is considered an endocrine disturbance which was probably due to frequent taking of vaginal smears.

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The Coöperative Solving of Problems by Young Chimpanzees

BY

MEREDITH P. CRAWFORD

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THE COÖPERATIVE SOLVING OF PROBLEMS BY YOUNG CHIMPANZEES[1]

MEREDITH P. CRAWFORD

From Yale Laboratories of Primate Biology

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I. INTRODUCTION

Coöperation is a type of social behavior probably displayed by most infra-human primates, yet studied in experimental isolation in none. The term has been used to describe a great variety of behavior patterns, some of which are only remotely related, so that definitions of coöperation range between wide extremes. On the biological side, Allee (1931) uses the term to indicate the condition of supplementation of foodstuffs and waste products between members of a plant or animal community. Coöperation is, for Allee, a fundamental property of protoplasm, varying in the complexity of its manifestations throughout the plant and animal kingdoms.

For the social scientist coöperation may have a much more limited meaning, as is indicted by Young (1934, p. 377):

> Coöperation, like opposition, rests upon deep seated features of the personality, which have grown up in the course of social interaction. There is no instinct of coöperation in the technical sense; it is distinctly a development of social living. The psychology of coöperation means nothing without reference to the social framework.

Coöperation means sympathy, identification of one person with another, and teamwork, the coördination and integration of action and not merely additive effort, according to Young. It arises from the orientation of the individual to the in-group and to the out-group, and is most apparent when the in-group directs activity against the out-group.

Hayes (1925, p. 340) has given a general definition of coöperation which seems to lie intermediate between the above extremes.

> Coöperation is the relation between activities that promote the same result. Nothing is indicated by the word coöperation as to the nature of the activities as such, but only as to the relation between them. Activities of the most extremely different kind may be in coöperation. . . . The only thing designated by the word coöperation is the relation between activities that contribute to a common result. Organization is a complex form of coöperation. The word denotes a two-fold relation, the relation of difference combined with the relation of coöperation when each activity is made more effective in promoting the common

result by its relation to the other and different activities. As soon as one actor begins to direct the activities of others toward a common result, his own activity is differentiated from the activities of the rest. And usually he directs some to do one kind of work and others to do other kinds. But coöperative differentiation may arise spontaneously without conscious planning. Indeed the development of the relationship of differentiated coöperation is the main feature of evolution.

Since coöperation may occur in so many situations, the actual behavior patterns involved may be extremely diverse. Hence, observations on particular examples are seriously limited in their application to closely similar situations. For an experimental study a situation should be so chosen that it may represent, as nearly as possible, a general case, the observations on which may apply to a large number of situations. Criteria for choice might include simplicity of individual tasks, easy recognition by the subjects of joint activity, and possibility for quantitative observation.

In the present experiment we limited our study of coöperation to a description of behavior patterns appearing in situations requiring teamwork—the coördinate activity of two individuals working for a common incentive object. The methodology employed involved two steps. First, young chimpanzees were trained individually in a problem situation until they had thoroughly mastered the manipulations required. Second, a slight modification of the problem apparatus was made so that one animal could no longer operate the mechanism by itself, whereas two animals, working together, could successfully perform the manipulations. These modifications in apparatus involved merely an increase in muscular strength required, or a greater spatial separation of parts which had to be operated simultaneously.

As experimental subject, the recent literature[2] indicates that

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[2] Summaries of the literature on human subjects working in coöperating groups, using the term in somewhat the sense we have defined it, have been given by Murphy and Murphy (1931, ch. 9) and by Dashiell (1935, pp. 1125–1140). The effect of group discussion upon performance of members of a group, as well as the total group output, represents a typical interest of the studies reported.

Studies on coöperative behavior in children have been of an observational in many respects the chimpanzee is an ideal animal. Nissen (1931) reports that in its natural habitat this ape is a highly socialized creature. In his account, in addition to a picture of the natural social groupings (on 25 occasions groups of from 4 to 14 animals were observed; the groups usually contained one or more mature males, mature females, juveniles, and dependent infants; of the animals observed whose sex was determinable 65 per cent were females and 35 per cent were males), observations are reported on play behavior, mother-infant relationships, types of communication, and apparent acts of social service, which suggest that the animal is capable of some complexity in social response. Nissen has made the point, however, that in the native habitat, where the necessities for life are very easy to obtain (food, water, simple shelter, and protection from enemies), the chimpanzee's full capacities are probably not taxed to the limit. The necessity for well integrated coöperative activity in order to satisfy biological needs perhaps has never arisen, and hence in the field one should not expect to see the type of teamwork which might be demonstrated in the laboratory, where such behavior could be put at a premium.

No studies on coöperation, in the limited sense of teamwork defined above, have come to the attention of the writer.[3] The use by Bingham (1929, pp. 76–77) and by Köhler (1925, pp. 166–172) of two or more chimpanzees together in the box stacking problem approximates, to some extent, the present experimental situation. Both of these observers found evidence for social facilitation of activity, and occasionally, one of Köhler's apes seemed to be lending a helping hand. Mutual obstruction and competition, however, seemed the typical behavior, where one animal could do the work if left alone.

There are, however, various laboratory studies on related forms

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nature, in which natural patterns of action, stimulated by various play objects, have been foci of attention (Updegraff and Herbst, 1933). No studies on coöperative problem solving, such as the present one, have been discovered in the literature for human subjects.

[3] A title has appeared (*Psychol. Bull.*, October 1936) indicating that a paper was read before the Midwestern Psychological Association by D. L. Wolfle on "Coöperative behavior in monkeys and children."

of coöperative behavior which should be mentioned, since they indicate the chimpanzee's capabilities in the coöperative direction. The reader is doubtless familiar with the quotation from Köhler (1925, pp. 282–283), which remarks the intensity of social forces binding chimpanzees one to another. Nostalgia, longing for companionship, and lack of social life may so incapacitate a chimpanzee as to render him almost useless as an experimental animal. Yerkes and Yerkes (1929: esp. chs. 20, 21, 24, and 30; 1935) have indicated certain areas of chimpanzee life where coöperative behavior, broadly defined, occurs, as for example, sex behavior, mother-infant relationships, play behavior, acts presumably involving social service, etc. In some of these areas controlled observations have already been made. Yerkes and Elder (1936) have described the mating behavior of chimpanzee, and have related the natural patterns of coöperative activity to certain physiological and psychological variables, following the preliminary observations by Bingham (1928). The study of the mother-infant relationship, rich in examples of coöperative acts between mother and infant, as well as indicative of natural modes of chimpanzee tuitional behavior, has been introduced in a paper by Yerkes and Tomilin (1935). Yerkes (1933) has isolated one of the natural social behavior patterns, grooming, and has indicated certain of its genetic aspects. In the food-sharing situation Nissen and Crawford (1936) have observed natural indications of mutual aid and dependence on the part of young chimpanzees. While the present specific problem in coöperative behavior has not been previously attacked, there is every indication that the chimpanzee should prove a useful and significant subject.

Interpretations of studies on the behavior of the sub-human primates, especially those of social behavior, are sometimes made in unjustifiable directions. Before proceeding with the present investigation it may be well to suggest two types of limitations of interpretation from species to species of data on social behavor. The first concerns the general biological relationships of the form considered, and the second its possession of culture. Comparative psychologists have stressed the fact that behavioral data are valuable for comparative purposes only if the systematic zoological positions and ecological connections of the animals studied are clearly recognized. Obvious structural, physiological, and environmental differences between various forms must be clearly understood as important limitations upon the application to other species of conclusions drawn from observations on one form. Of course, the comparative psychologist is interested in behavior wherever he finds it, and to the extent to which its formal patterning is similar in various organisms, it may be considered analogous. Yet he must be constantly on his guard against an uncritical crossing of biological gaps. In the present instance, the facts accumulated about the social behavior of chimpanzees may be considered not only interesting in their own right, but suggestive of similar facts which might, with proper methodology, be established for other species, including the human being.

The possession of culture by man, in apparent contrast to the ape, adds a special limitation to the comparison of data on the social behavior of the two species. This peculiar limitation has appeared so great to some as to render any attempt at a comparative social study decidedly unprofitable. However, a *bio-social matrix* of social behavior, common to ape and man, and probably to a certain extent to all primates, can be postulated, which may be regarded as the biological foundation upon which culture rests (see Warden, 1936). It is with the biologically determined primate social patterns included in this matrix that we may be concerned in the study of infra-human primate social behavior. We may leave the troublesome problem of tracing relationships between it and cultural specializations until more data on both sides are available.

II. GENERAL PROCEDURE

The aim of this investigation was twofold: to discover what natural modes of attack young chimpanzees might employ when presented with certain problems for coöperative solution, and to train them in the solution of such problems. Animals, who had first been trained individually to manipulate certain pieces of apparatus, were introduced in pairs to the same apparatus, modified so that it could no longer be operated by a single animal. After observation of the natural attacks of the pairs of chimpanzees, certain training procedures were instituted.

Three problem situations were employed: *Problem I, box-pulling*, which required the pulling in of a weighted box, too heavy for a single animal to move, with two ropes; *Problem II, two-cord problem*, which required the opening of the door of a food box by simultaneously pulling two vertically suspended cords; and *Problem III, double-handle problem*, which required the operation of two handles in a coördinated manner to bring a food tray within reach. Problem I proved most useful because of the small amount of individual training necessary for each subject preliminary to work in pairs, the length of time required to draw in the box, and the simplicity of the situation for the experimenter's observation. The apparatus and method employed for each of these problems will be described as preface to the presentation of results with each. In this section certain points of general procedure, common to all the problems, will be treated.

During the early part of the experiment, various methods of recording the multiform qualitative observations made on behavior in these problem situations were put to test. The rather elaborate procedures used by observers of social behavior in children (see Thomas, 1929) suggested one technique. Record blanks were ruled to represent short intervals of time, and a code of symbols covering most of the types of activity likely to be observed, positions about the cage, names of animals, etc., was prepared. During an experimental session the observer followed in detail the behavior of each subject, making entries in code on the prepared sheet at intervals of ten seconds. These records were later decoded and summarized in typewritten form. This method proved expensive of time for transcription, unnecessarily detailed, and relatively inflexible, and was abandoned in favor of a simpler procedure.

The technique finally adopted consisted simply in writing in longhand an account of the behavior as it occurred, or a few seconds thereafter. These descriptions were edited and supplemented immediately after each period of work. A dictaphone was always ready for instant use, and was employed when significant behavior occurred too rapidly to be written. These recordings were transcribed and included in the experimental notes. All the data collected during this experiment are probably subject to a certain amount of error through failure to see all the relevant activity of both animals, inaccurate descriptions,

TABLE 1A

The sequence of experimentation with various pairs of subjects during the spring of 1935

ROSS AND BULA	KAMBI AND ALPHA	ROSS AND ALPHA	KAMBI AND BULA	BIMBA AND BULA
I no. 1–51 3/18–5/11				
	II no. 1–3 4/24–29			
	III no. 1–21 5/20–6/12			
no. 52–65 5/27–6/10				
	II no. 4 6/12			I no. 1–7 6/11–14
no. 66–70 6/17–20				
	I no. 1–15 6/14–24			
III no. 1–9 6/19–24				
	III no. 22 6/24			
I no. 71 6/25		III no. 1 6/25	III no. 1 6/25	

The Roman numerals indicate the problem situation used, as Problems I, II, and III. The Arabic numerals refer to the sessions. Sessions on each problem are numbered in separate sequences. Inclusive dates for each group of sessions are given. The spacing of each group on the table indicates roughly the sequence of pairing.

TABLE 1B

The sequence of experimentation with various pairs of subjects during the fall and winter of 1935–1936

BULA AND BIMBA	BULA AND KAMBI	BULA AND ALPHA	BIMBA AND KAMBI	BIMBA AND ALPHA	KAMBI AND ALPHA
1935					
I no. 1 9/16	I no. 1 9/17				I no. 1 9/12
		I no. 1–5 9/23–26			
no. 2–10 10/16–11/20	no. 2–55 10/9–12/21				
				IV no. 1–17 10/21–11/25	
		no. 6–12 10/30–11/5	I no. 1–7 10/30–11/5		no. 2–4 11/7–15
		no. 13–17 11/18–25	no. 8–13 11/18–29	I no. 18–36 11/26–12/21	
IV no. 11 11/30		IV no. 18 11/29			
					no. 5–17 12/2–12/17
I no. 12–28 12/2–20					
1936					
I no. 29–36 1/2–11	I no. 56–57 1/3–4			I no. 37–38 1/3–4	I no. 18–56 1/3–2/21
II no. 37–51 1/14–31		I no. 19–26 1/6–14	I no. 14–21 1/6–14		
	no. 58–59 1/15–16			no. 39–40 1/15–16	

TABLE 1B, 1936—*Concluded*

BULA AND BIMBA	BULA AND KAMBI	BULA AND ALPHA	BIMBA AND KAMBI	BIMBA AND ALPHA	KAMBI AND ALPHA
		no. 27–28 1/17–18	no. 22–23 1/17–18		
	III no. 60–68 1/22–31			no. 41–45 1/21–27	
				IV no. 46–48 1/28–30	
				I no. 49 1/31	
	I no. 69 2/1				
				II no. 50 2/3	
	III no. 70–74 2/3–10				
				I no. 51–52 2/5	
				II no. 53 2/7	
			III no. 24 2/13		
III no. 52–55 2/14–17					
V no. 57–65 2/18–24					

The Roman numerals indicate the problem situation used. Problems I, II, and III are described in this report; IV and V will be reported elsewhere. The Arabic numerals refer to sessions, which are numbered continuously for each pair of animals, regardless of the sequence of use on more than one problem. The spacing of each group on the table indicates roughly the sequence of pairing.

and bias in the selection of types of behavior for close observation. Other observers were present from time to time, who corroborated certain of the observations. Moving pictures were taken at intervals for use in illustrating significant types of behavior and to provide a partial though permanent type of uninterpreted data (Crawford and Nissen, 1937).

The experiments were conducted during the spring of 1935 and during the fall and winter of 1935–1936, in the Northern Division of these Laboratories in New Haven. The work in the spring was of preliminary and investigatory nature, while that during the fall and winter followed a more systematically outlined course. The schedule of spring experimentation is presented in table 1a, and that for the fall and winter in table 1b. In each of these tables the columns represent the pairings of subjects. The work is indicated in *sessions*, which represent the units of experimentation with each pair on each day. A session lasted between fifteen and thirty minutes, and included a variable number of *trials*. The term trial is used to designate one successful operation of the problem apparatus from which each animal received food. Each subject was generally used in more than one pairing every day, so that, for example, Bula might have worked one session in the morning with Kambi and one in the afternoon of the same day with Bimba. In the tables the sessions are grouped into chronological sequences, and the groups are arranged in staggered order, so that, with the aid of accompanying dates, the reader may trace the shifting of partners. In table 1b, dates are included for Problems IV and V, which are not reported here. These problems were of considerably different nature from the ones described in this paper, and training on them seemed to have no effect on the behavior of the subjects in Problems I, II, and III. They are included in the table in order to give a complete record of the coöperative experimentation of the subjects during the time covered by this report.

III. SUBJECTS

Five young chimpanzees served as subjects in this experiment. Ross was used during the spring of 1935, but was replaced by Bimba for the work during the fall and winter of 1935–1936, so that the final group included Bimba, Bula, Kambi, and Alpha. A brief life history, with particular emphasis on social relationships, is given below for each subject.

Ross, male. Hypothetical birth date, November, 1929. Transferred from Johns Hopkins Medical School, June, 1933.

Ross was a good natured, playful, relatively stupid chimpanzee, socially dependent, tending to take the submissive attitude, and capable of forming close friendships with other animals, especially those smaller than himself. When first brought to these Laboratories he was caged with other larger males. They worsted him in many fights so that he was kept in isolation for a time. In November, 1933, he was placed with a smaller female with whom he immediately made friends. Subsequently he was always kept with smaller animals toward whom he was not aggressive. During the early part of 1934 he was caged with Bimba and Frank, and later with Bula and Frank. With both the females he formed close relationships. In each case the female took the dominant rôle, treating Ross in a motherly fashion. The alliance with Bula was particularly strong and lasting and was strikingly manifested by the hours which they spent in tandem-marching,[4] and by the willingness with which they shared food. The food sharing was done largely by Bula in response to begging by Ross. When, in 1935, Bimba and Bula were both living in the same cage with Ross, they were both friendly with him and seemed to show a mild rivalry for his companionship. Bula was usually Ross' favorite. It was not until after nearly two years of life in the colony that Ross became a useful experimental subject. His first work was in weight discrimination where he proved a willing but not accurate subject. Food-sharing was the only other experiment in which he had been used previous to this one. Ross died during October, 1935.

Bula, female. Born on the Abreu estate in Cuba, April 27, 1930.

Bula is a self-confident, active chimpanzee, strong and agile, though not large for her age. She participates somewhat less than the other animals in group romps and scuffles, though she has the capacity for making close friends. Separated from her mother at the age of one year, she has been living with other animals ever since. She was transferred from the Southern Division at Orange Park, Florida, to New Haven, at the age of eighteen months. Toward the end of her fourth year, Bula was quartered with Alpha and an aggressive young male. The two females learned to come to each other's defense against his attacks. During this period Bula's personality showed a change from aggressiveness and independence toward docility and social dependence. She usually seemed eager to come to work in the experiment room. Bimba and Bula have been good friends throughout life, and on the death of Ross, Bula's particular companion for more than a year, the alliance between Bimba and Bula was reinforced and continued strong throughout this experiment. Bula has been judged of better than average intelligence. She had served as subject in seven experiments up to the beginning of this investigation.

---

[4] *Tandem-marching* refers to the habit of two or more chimpanzees placing their arms about each other and marching about the cage in lockstep.

Alpha, female. Born in the Yale Laboratories of Primate Biology, September 11, 1930.

Alpha, the first chimpanzee born in these Laboratories, was immediately isolated for study of development (Jacobsen, Jacobsen, and Yoshioka, 1932). After nine months she was placed with Bula, then fifteen months old, whom she dominated in a friendly manner. Both animals were brought to New Haven where they lived in various combinations with the two infants Bimba and Kambi. Particular friendships were formed between Kambi and Alpha, and between Bimba and Bula, though they all played together without antagonism. She was used in six experiments previous to this one.

Alpha is an independent animal, not aggressive, yet able to defend herself against her fellows. She has been judged to be of slightly below average intelligence by a group of observers. She is very timid when placed in a new experimental set-up, and seems peculiarly dependent on the experimenter's immediate presence for assurance. During this experiment, Alpha never seemed quite at ease as long as the observer was concealed within the observation booth. She frequently sought the the other animal for comfort in tandem-marching. This fear of being left alone doubtless had an effect on the results with Alpha, who made least progress toward learning to coöperate. During the fall of 1935, she became difficult for observers to manage, though by the end of the year her characteristic friendliness and tractability had returned.

Bimba, female. Hypothetical birth date, August, 1929. Brought from Africa in 1930.

Bimba and Kambi were brought from Africa in 1930, and were cared for together for more than a year. They were strongly attached to each other and mutually dependent in a manner characteristic of infant chimpanzees. After Alpha and Bula were added to the colony in New Haven, these four were caged together at various times. The alliances shifted, and Bimba made friends with Bula, while Kambi and Alpha joined forces. Bimba and Bula were both very much attached to Ross, whom they dominated and mothered. Bula seemed first in his preference. After Ross' death the Bimba-Bula friendship was strengthened, so that during this experiment they were difficult to separate.

Bimba has been rated one of the most intelligent animals in the colony, and has demonstrated her ability in six different experiments. She is lightly built, apparently not strong, although quick and active, and skillful in motor manipulations.

Kambi, female. Hypothetical birth date, June, 1929. Brought from Africa in 1930.

Kambi was brought from Africa with Bimba and lived with her during the first year of life. During her third year Kambi and Bimba were caged with Bula and Alpha. All four lived together, with occasional isolation of one of the four. Kambi and Alpha became friends, though their relationship was never as close as that between Bimba and Bula. Kambi is an independent animal, who tends to take the dominant rôle in a group, although she usually does not start fights. She is of phlegmatic temperament, and is rated of average or subaverage intelligence. At the time of the experiments there were indications that she was approaching sexual maturity. She spent a great deal of time sitting in a corner and swaying back and forth, probably stimulating her genitalia thereby, a habit which might be considered a form of masturbation. Kambi had been used in five experiments previous to this one.

During the period of this experiment the subjects were grouped in their living cages in various combinations. Changes were made in the groupings from time to time in order to maintain optimal social relationships, and especially to prevent spread of infection during times of illness. The friendships indicated in the life histories above were maintained throughout the experimental period. Since each subject lived with each of the other subjects at some time during the year of this experiment, it was possible to obtain some indication of the probable social dominance relationships obtaining between them (see Zuckerman, 1932; Maslow, 1936). In order to get some rough measure of these, six laboratory staff members, intimately acquainted with the animals for a year or more, were asked to rank four of the subjects in order of dominance. (Ross was not living when the ratings were made.) They limited their consideration to the living-cage situation, and used food-division and outcomes of fighting as criteria. The average position assigned each subject by the six raters is presented in table 2, together with the weights at different dates, and the birth date. It appears from this table that Alpha was the most dominant, followed by Kambi, Bimba, and Bula, in that order. Because of their large variability, these ratings can be accepted only tentatively and used in suggesting interpretations of the experimental results. A rough relationship between dominance position and size, but not age, may be observed.

TABLE 2

Average position of subjects in a probable dominance hierarchy

SUBJECT	DOMINANCE RATING		WEIGHTS			BIRTH DATE
	Average	A.D.	Apr. '35	Oct. '35	Jan. '36	
			kgm.	*kgm.*	*kgm.*	
Alpha	1.3	0.4	23.6	25.2	28.1	September, 1930
Kambi	2.3	0.7	23.8	22.2	27.4	June, 1929*
Bimba	2.8	0.9	20.0	19.8	22.6	August, 1929*
Bula	3.5	0.5	21.9	22.5	25.5	April, 1930

* Hypothetical birth date.

IV. APPARATUS AND METHOD FOR PROBLEM I, BOX-PULLING

The general plan of the experiment room, restraining cage, and observation booth is shown in figure 1, together with the apparatus for Problem I. The framework of the sides of the *restraining cage*, whose overall dimensions were 2 meters in length, width, and height, was made of wood covered with hardware cloth (mesh approximately 1 cm. square, 16 gauge wire). The two doors at the rear of the cage and the partition which divided the cage longitudinally into halves were of the same construction, while the roof was made entirely of wooden planking. At the forward end of each half of the cage and in the rear part of the partition, grilles of metal bars were placed which extended from the floor to a height of 80 cm. The cold rolled steel bars, 13 mm. in diameter, in these grilles, were spaced far enough apart to allow a chimpanzee to reach its arm between them as far up as the shoulder. The grilles in the front wall of the cage will be referred to as the *front grilles,* and that one in the rear of the partition as the *back grille.* The front grilles were fixed, while the back one could be taken out to allow free passage for an animal from one half of the cage to the other. Throughout most of Problem I, the back grille remained in place, and only the left half of the cage was used by both of a pair of subjects.

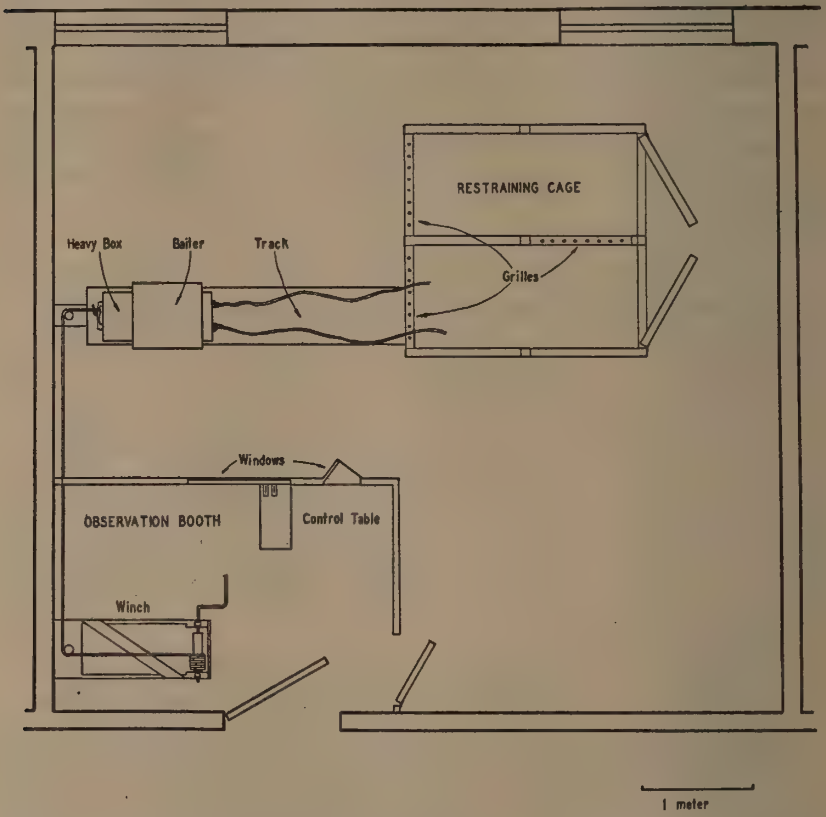

Fig. 1. Floor Plan of the Experiment Room with Apparatus for Problem I

The *observation booth* was built of wallboard on a wooden framework. It completely enclosed one corner of the experiment room including its entrance door, which location made it necessary to bring the animals through the booth to the experiment cage. The observer, after placing the animals in the cage, retired to the booth and closed the wallboard door, so that he was completely concealed from the animals. Since he closed the door through which the animals had last passed, his retirement into the booth may have contributed to an illusion that he had left the room entirely. Due to its particular placement in the room, additional observers could enter the booth during the experiment without disturbing the animals. Two one-way-vision screens (*windows*, fig. 1), one made of wire screening and black cheesecloth, and the other of a partially silvered mirror, were placed in the wall of the booth. A small table (*control table*) stood under one of the observation screens or windows. On this certain electrical control devices were mounted, and records were written. A dictaphone stood in the booth ready for instant use when the observer wished to make a detailed record of the behavior as he observed it. The restraining cage and the observation booth remained as described for Problems I, II, and III.

The essential apparatus for Problem I comprised a wooden track on which the *heavy box* was drawn by two ropes. The *track*, which was 50 cm. wide and made of wood, extended from the front grille of the cage to the wall of the room. The box was made of wood (28 x 45 x 100 cm.) and was divided into two compartments, the open rear compartment being used to carry weights. The box itself weighed 60 pounds, and a total weight of 350 pounds in pieces of lead and other metals was available for loading into the rear compartment. The coefficient of friction of the box sliding along the track was approximately 0.45, so that a horizontal pull of about 180 pounds was required on the ropes to move it when maximally loaded. For the recording of amount and duration of pulling on both ropes, two spring balances (with scales from 0 to 300 pounds) were securely mounted in a wooden framework bolted to the bottom of the forward compartment of the box. One rope was attached to each of these balances. To the indicators of each balance a metal stylus was attached which traced a line on stylograph paper as it was moved by a telechron motor. Thus two lines were made from which the exact amount of pulling on each rope at each instant could be determined. A third line, which served to indicate when the observer saw the box begin to move, was made by an electromagnetic marker, controlled within the observation booth. Typical records made by this recording unit are illustrated in figure 2, pp. 46–47.

The electrical mechanism for delivering food to the top of the box was called the *baiter*. It formed a bridge over the far end of the track, under which the box was withdrawn from the cage by the *winch* inside the observation booth. Delivery of food from the baiter was effected by an endless belt mechanism. Inside the baiter were two belts running over wooden cylinders on two transverse shafts, one of which was driven by an electric motor. Pieces of food were placed at intervals along the two parallel belts, and when the motor was started the tops of the belts moved forward so that the food fell off the ends, over the forward wooden cylinders, through holes in the floor of the baiter, to the top of the heavy box below. One or more units of food could thus be placed on the box by applying current to the motor for a few seconds. The fall of the food upon the box served as a cue for the animals to begin pulling. The mechanism of the baiter was so enclosed as to be removed from view of the animals, although visible to the experimenter within the observation booth, where its controlling switch was placed.

For the early part of the experimentation during the spring of 1935, apparatus for Problem I was much simpler than that described. A box without recording mechanism was used; the experimenter baited the box by hand, and usually remained in front of the cage during most of the trials.

The individual training of each subject for Problem I was very simple, since the pulling in of a box with a rope presented almost no problem for the animals. A few days were spent in individual pulling, during which the weight of the box was gradually increased until the animal no longer pulled it readily. No attempt was made to determine the maximal load each subject would pull under these conditions. Enough weight was added, however, to require the apes to brace themselves against the grille bars, and to pull with both hands, employing arm, leg, and back muscles.

V. RESULTS WITH PROBLEM I

The chief data of this experiment are the qualitative records of each subject's learning to coöperate with a partner. Summaries of the training of each pair of animals have been prepared from the daily experimental records. The behavior of the animals when first introduced into the problem situation was entirely uncoördinated, and only after special training were they able to combine their efforts to draw in the box. Three stages in the development of solutions to this problem have been distinguished, each one representing a partially adequate solution. The first stage was characterized by the simultaneous pulling response of the animals to an external cue provided by the experimenter, which, because of the simultaneity of the behavior, effected coördination. The second stage was recognized when one member of a pair became independent of the experimenter for his cue for coördinating, and instead watched the other animal pull and timed its pulling with that of the partner. The third stage appeared when an animal, with manual gestures, solicited from the partner help in pulling.

In this section summaries of the training of four of the seven pairs of subjects used in the experiment are presented. The first is for Ross and Bula, the original subjects trained, whose records trace the course of the initial training. Experimentation with this pair was necessarily discontinued after Bula had mastered stage 2, in which she watched and coördinated with Ross. The second and third summaries, for Bula and Kambi, and Bula and Bimba, trace particularly the development of stages 2 and 3, in which both Bimba and Bula solicited their partner's help in pulling. In the fourth summary a brief account is given of the observations on Bimba and Kambi, who never learned to pull together, even at stage 1. In these summaries excerpts from the original records are frequently inserted in order to give an account of the behavior just as it was observed. Summaries for the other pairs of subjects are omitted, since they do not add significantly to the data already presented.

Following these four summaries certain typical samples from the graphic records of pulling are presented. These reveal important facts about the amount, timing, and patterning of pulling of the subjects in all combinations.

A. Bula and Ross

Ross and Bula were the first two animals used in this experiment, and were started during the spring of 1935 on problem I, box-pulling. At the time experimentation began and for the first sixty-six sessions thereafter, the finished apparatus as completely described above was not in use. The heavy box was not equipped with recording mechanism, there was no mechanical baiter, and the box was drawn back by pulling it directly from behind. For the greater part of the time the experimenter remained within sight of the animals and not in the observation booth.

In this preliminary and exploratory investigation, we were attempting to discover (1) what natural responses the chimpanzees would make when presented with the heavy box and two ropes for drawing it in, and (2) to what extent, failing immediate solution of the problem, the animals might be trained to work together at the task. The procedure was subject to change whenever it seemed advisable in order to speed up learning. The total coöperative act had to be broken up into many parts for training. It is not intended to imply that the preliminary procedure used here was the best or the most efficient method for training new subjects in coöperative work. It was indicated, however, that the fundamental stages through which any training of animals must pass would correspond roughly to the ones encountered here.

When Ross and Bula were introduced into the situation immediately after individual training with a single rope, their efforts toward moving the box were entirely uncoördinated. There was no suggestion in the behavior of either which would indicate any attempt of one to coördinate its activity with that of the other. While their efforts were directed toward the food-baited box, and while they both made strenuous efforts to draw it in alone, they never, even for an instant, pulled simultaneously. Typically, one animal would pull while the other watched or played about the cage. When the first finished pulling, the other would take the rope, often the same one, and pull while the first animal did something else. Frequently they took a succession of turns at the ropes in this manner. A typical record sheet for these early experimental sessions is presented below.

Session 5. Animals placed in cage and both ropes presented by experimenter. Ross pulls lightly on left rope (RL), drops it, and marches in tandem with Bula about cage. After marching twice about cage (50″),[5] Bula breaks away and pulls lightly on RL twice while Ross stands by watching. Ross then picks up right rope (RR) (1′ 05″) and pulls it, after Bula had dropped RL. Bula picks up RL again and begins to adorn[6] herself with RR. Bula shakes RL as if she were trying to shake food off the box. Both drop ropes and march about cage until 1′ 50″ when they separate and Ross picks up RL, drops it, and picks up RR, and Bula picks up RL. Ross pulls RR (2′ 10″) while Bula drops RL and goes to back of cage. Ross drops RL, moves to another part of cage, returns and begins to adorn himself with RR (2′ 30″). While Bula remains in rear, Ross pulls on RR, then drops it (2′ 50″) and Bula comes up immediately, picks up, pulls, and shakes RR. Ross has gone away and climbs about on opposite cage grille, and Bula, dropping her rope, walks about alone. Ross returns to grille (3′ 20″) and pulls RL lightly, while Bula sits in the middle of the cage scratching. Bula moves toward Ross and they join and begin to march, continuing until 4′ 10″, when Bula picks up RL and pulls it (4′ 30″) while Ross walks about alone. He picks up RR, adorns himself with it, then pulls it (4′ 40″). Bula comes up and pulls once on RL for an instant after Ross has dropped RR (5′ 00″).

During the first nine sessions uncoördinated behavior continued until the animals were removed at the end of each fifteen-minute period. Incentive was added to the box twice during each session. On each addition more vigorous pulling was done by each animal, but no coördinate pulling appeared.

Since each of the animals pulled in a more heavily loaded box when in the cage with another animal than when alone, the factor of *social facilitation* is suggested. While the reality of its operation is hardly doubted, unfortunately the preliminary training was not planned in such a way as to obtain a measure of this factor.

The uncoördinated activity typified by the report of session 5, quoted above, continued through session 8. The box was increased in weight, making it more difficult, though not impossible, for a single animal to move. It seemed to be indicated that the animals would not chance upon any method of coöperative pulling if left to their own devices. Beginning with session 9, two light boxes were introduced, with a rope attached to each, in order to stimulate the animals to work *simultaneously*, even though not coördinately, at the task of pulling.

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[5] Time in minutes (′) and seconds (″) entered for various activities.

[6] Draping the rope around neck and shoulders.

Presentation of two boxes, or two boxes followed or preceded by one box, constituted the routine of a majority of the sessions between numbers 9 and 32 (April 12 to 29). Training was necessary even for the development of simultaneous activity for different objectives. For example, Ross pulled in both the boxes on the first two trials, while Bula sat watching him. It was not until the twelfth and thirteenth sessions that the animals were pulling even the two boxes at the same time. On the sessions when the animals were able to pull the single box together, the pulling seemed exactly like that shown with the two boxes; they seemed to be working at two different objects. They worked rapidly without apparent attention to each other's activity.

This type of pulling together, even when it resulted in bringing in the box, was typical of all the pulling together through the eighteenth session. On the nineteenth session, when one box was used, the pulling of the two subjects seemed to synchronize; that is, they began their heaves at about the same instant, pulled for about the same time and distance, took up slack together, and were ready to begin the new heave together. Inasmuch as neither animal was observed to watch the other during these rhythmic pulls, we suspected that the synchrony arose from one or both of two sources: (1) the animals probably had about the same natural individual speed of pulling and of taking up slack; and (2) a slight movement of the box by one animal would supplement the pull of the other and cause the box to move forward, so that after this little forward motion of the box a continuance of pulling by both animals would result in a finished stroke. Since this rhythmic pulling broke down within a few trials, particularly when the heavy box in session 25 was encountered, it will be regarded tentatively as an artifact of the light-box situation and the uncoördinated but simultaneous activity of the two animals.

Sessions 26 to 33 represent a transitional stage between the mastery of the habit of simultaneous pulling and that of responding to a cue given by the experimenter. During this transition there appeared the first indication of the behavior characteristic of the next stage of learning; that is, one animal was observed to *watch*, for a few seconds, the activity of its partner. While this had been noticed twice before, its first clear-cut appearance was made when Bula and the experimenter were pulling together with two ropes. No session by session account will be given of the training during this period, since a general summary will suffice to indicate the main trends.

After a few minutes of uncoördinated activity on session 26 (April 24), the animals were removed and then brought back to the cage alone, one at a time. The experimenter entered the cage with each in turn and pulled on one of the ropes with each, drawing in the box a number of times. This procedure was repeated on the twenty-seventh and twenty-ninth sessions. On each of four trials of session 26, Bula was observed to look around at the experimenter just before pulling or during the course of pulling in the box. On the last trial she indicated her awareness of the experimenter's function as a helping partner in the following behavior:

> Trial 4. Bula did not come to the cage door when I came in, but went directly to the grille. She pulled a couple of times on both the ropes together, then moved over and gave me a place to sit down. I took one rope and she the other. She waited a moment until I was set in pulling position and then began to pull.

Ross, on the other hand, seemed to pay very little attention to the experimenter when he entered the cage to pull with him. It did seem as though he was pulling somewhat more actively when the experimenter was in the cage with him than when he was alone. Perhaps he had some vague realization that the presence of the experimenter made box-pulling easier, or perhaps a general social facilitative effect was in operation.

In session 28 the effect of the training with the experimenter was not immediately apparent. For the first four minutes after two ropes (from the 370-pound box) were presented, the pulling was entirely uncoördinated. Then the ropes were re-presented and the experimenter helped them both pull with the following results:

> (4′ 00″) I went to grille and presented the two ropes to the animals and remained in front of grille holding a hand of each on one of the ropes. I pulled a little on Bula's rope as Ross was pulling on his, and the box moved a little. Then, after a few more presentations, I turned around with my back toward the animals and pulled on both ropes and shook the ropes to be sure that each animal was holding its rope. We all three pulled, the animals doing most of the pulling and Ross doing the greatest share. The box was drawn in by short heaves in about 30″. On the last two heaves the animals were pulling it entirely alone. Ross would begin pulling and pull hard, while Bula would wait until the box was just about to move, would begin her pull, and the box would move.

The experimenter continued in this rôle of puller and coördinator for a number of sessions following. The pattern of behavior was much as is described above: Ross pulled first and hardest, and assisted by the experimenter he was able to move the box a little. At the instant the box moved Bula tightened the slack rope which she had been holding and joined the heave so that they pulled together. At this stage it appeared that Ross was responding partly to the efforts of the experimenter, and Bula to a complex of stimuli: the experimenter's pulling, Ross' rope tightening, and the first movement of the box. In the following sessions, will be traced the successive reduction of cues from this original complex to which the animals learned to respond.

For a few sessions after number 28, the coördination was not apparent. On number 29 the experimenter again worked with the animals alone with results as described above. Sessions 30 and 31 were entirely uncoördinated, and on session 32 the two boxes were used again for the last time. Four trials with the two boxes were given during which each animal pulled in its own box rapidly and obtained the food thereon. When the single box was presented again, it was necessary for the experimenter to assist the subjects in order to stimulate simultaneous pulling.

On the following trials the experimenter continued to give assistance, at first by helping with the pulling of the ropes. Later he moved behind the box and pushed from there. He found, on successive trials, it was necessary to do less and less pushing, until finally he was helping only with the first or first and second heaves. On the fifth trial of session 35 a verbal command of "pull" was given when one of the partners began to pull. It was attempted to give the cue in as regular cadence as the working of the animals would permit. There was evidence as the training progressed that the animals, particularly Bula, responded to the verbal cue by beginning to pull. The typical behavior described above continued: Ross pulled first, followed by Bula after a second or two. The call of the experimenter often served to hasten Bula's response. The first clear-cut response by Bula to the call appeared on trial 4 of session 37.

On the third trial of session 40 the experimenter did not do any pushing at all. He presented the ropes, retired to the end of the track, and began a rhythmic calling to which the animals seemed to be responding as they moved the box in by their own coördinate efforts. At this time Bula was often observed to look back over her shoulder just before pulling, as if she were afraid that something would come up behind her. The experimenter's call sometimes served to recall her to the task of pulling. After the response to the call had been well established, the experimenter was able to summon Bula to the grille and start her to work when she had left it. On the forty-third session the call was omitted on all the heaves except during two of the trials. On other trials a call served to get the animals started, after which they continued the rest of the heaves in cadence without the repetition of any extraneous stimuli. Sometimes pulling would go along for a few heaves without any call, and then the synchrony would break. Usually the command, "pull," was responded to by both, which established the cadence again.

On the forty-fourth session (May 6) the animals drew in the box seven times (trials) with only two commands from the experimenter. These were given at times when it appeared that the coördination was about to break down. During the next group of sessions (45 to 52), no significant change took place in the behavior. The coördination was a little smoother and the box was pulled in more rapidly without dependence on cues from the experimenter. The experimenter remained in front or at the side of the cage in view of the subjects. Some of the sessions were used in preparation for taking moving pictures (adaptation to special lighting conditions, camera, etc.). The pictures were taken successfully without serious disturbance of the coöperative activity.

By the time the verbal command was eliminated Bula had been observed to respond to various cues given by her partner, and thus was enabled to join him in pulling at the proper time. For a long time she pulled only after Ross had begun to pull. At first, when the box was light enough for him to move, she waited until the box moved before working. Gradually she began to pull when his rope tightened. Sometimes the stretching of his rope made enough noise to serve as an auditory cue to call her attention to the activity when she was looking away. First, she learned to respond to the movements of his rope. Second, with increasing frequency, she was observed to take her rope and brace herself in position to pull, look at Ross and wait until he was ready to pull, and then turn away and begin pulling when he did, or shortly thereafter. The length of time during which she would look at him as well as the number of times she looked increased markedly as experimentation continued. This "watching" behavior, some of which has been successfully photographed on moving picture film, has been considered the distinguishing characteristic of the second stage in learning to coöperate, in which the animals first showed a response to each other.

It must be pointed out that during an experimental session "watching," "response to verbal command," or any other of the typical behavior patterns which we have chosen to emphasize, did not all occur. The sessions have been characterized only as they showed a preponderance of one sort of behavior.

To summarize the training of the original pair to this point the following data are presented: fifty-two experimental sessions were given at the rate of once or twice a day, omitting certain days, from March 18 to May 27, 1935. During each session the box was drawn in from 0 to 8 times. After the animals learned to respond to the verbal cues, the sessions contained uniformly from 5 to 8 trials. From $\frac{1}{16}$ to $\frac{1}{4}$ of an orange or banana was obtained by each animal on each trial. In general, larger incentive seemed to stimulate greater activity than smaller. During the later sessions the box was weighted with 370 pounds and required a horizontal pull of 180 pounds to move it. It was drawn in by the two animals in from twenty to thirty-five seconds. Between sessions 28 and 52 a total of 103 trials was given the animals during which the box was pulled in by both animals together. Except during a few of the early sessions the box was never light enough to be pulled in by one animal alone.

TABLE 3

Presentations of one and of two ropes

SESSION	DATE	TRIALS WITH TWO ROPES BEFORE ONE	TRIALS WITH ONE ROPE	TRIALS WITH TWO ROPES AFTER ONE
53	5/27	4	1 F 6½′	1 T
54	28	3	1 F 5′	1
55	28	3	1 T*	
56	29		2 T*	1
57**	29		2 T*	1
58	30		1 F 6′	
59	30		1 F 5′	2
60	31	5	1 F 5′	
61	31	3	1 F	
62	6/1	1	1 F T	
63	5	2	1 F 10′	
64	10	4	1 F	1 F 4′

F indicates failure to pull in box in time allotted, indicated in minutes.

T indicates help or training given by experimenter, either inside or outside the restraint cage.

T* indicates the experimenter went inside the cage and pulled on the rope with Ross, Bula doing nothing.

** On session 57 a piece of timber was fastened across the floor of the cage so that one animal could stand behind the other and brace itself on the timber. Previously the smooth floor of the cage allowed no purchase to a second animal standing behind the first. In the two-rope situation each animal braced its feet on the grille of the cage.

When the coöperative activity had been stabilized for the two ropes in the manner indicated, a single rope was introduced instead of the usual two in order to discover whether the animals would, of their own accord, pull in the box by working together on one rope. During these sessions, the box was still too heavy to be moved by a single animal. Eleven experimental sessions were devoted to this part of the experiment. Trials were given with one and with two ropes in more or less balanced order during the sessions. In table 3 the results are indicated.

It is apparent from the table that at no time did the animals succeed in pulling in the box with one rope. They never gave any indication of trying to work together on it. The pattern of working at the rope in turns, described for the early trials with two ropes above, was characteristic of this behavior. The training received with the two ropes and the coöperative activity learned seemed to be specific to the two-rope situation, and was not in the least transferred to the one-rope situation. The anmals were probably hampered in the one-rope situation by their own strong preferences for working at a particular side of the grille, and by the difficulty of one animal's bracing itself when attempting to pull behind the other. In the program of experimentation, time did not permit of further training on the one-rope situation.

Sessions 65 through 71 were employed in reëstablishing the smooth coördination with the two ropes which had been temporarily broken down during the training with one rope. (Note the failure on the final two-rope trial in session 64 shown in table 3.) The characteristic behavior, with watching by Bula, reappeared on the sixty-fifth session, and continued through the seventy-first.

On session 66, May 17, the new box equipped with the recording mechanism (see description of apparatus) was introduced. The animals were at first disturbed by the spring action of the recording balances to which the ropes were attached, and a few trials were necessary to overcome their apprehension. On sessions 68 through 70, records were made of the amount and duration of each animal's pull.

These records confirmed an impression gained when the old box was in use, namely, that Bula did the greater amount of pulling, although she usually waited until Ross had started before beginning to pull. An analysis of the records for session 68 indicates that on the average Ross delivered 79 pounds pressure, and Bula 121. On the succeeding sessions the ratio was about the same, except for number 71, when Ross' average pull went to 90. On session 71, however, the ratio was reversed: Ross delivered an average of 149 pounds, while Bula delivered only 98.

Some of the records were analyzed for sequence of pulling. Of the 62 heaves, records of which were studied, 30 were made simultaneously by both animals (or so nearly so that it was impossible to tell which animal began first), while Ross began first on 29 and Bula on only 3 of the remainder. This lag in Bula's pull was approximately from .5 to 1.5 seconds.

At this point experimentation with these animals was discontinued. When they were paired together again in September, after no intervening training, they immediately pulled together without assistance or cues from the experimenter. Bula showed her characteristic watching of Ross. The position habits were still pronounced, Ross always pulling with the left, and Bula with the right rope. Ross died on October 3, and his place was taken by Bimba, in the fall and winter experimentation.

B. Bula and Kambi

Bula and Kambi were used during three experimental sessions in the spring of 1935, after Bula had worked successfully with Ross for almost eighty sessions and Kambi had been paired with Alpha for fifteen. Although intensive training was given, this latter pair did not learn to work together at that time. The experimenter helped Bula and Kambi during the spring trials, either by pulling Kambi's rope or pushing the box from the rear. A few trials were successful without aid from the experimenter. Bula watched Kambi a few times, as she had learned to watch Ross, and from the records it appeared that Kambi was leading in the pulling by 0.5 second, Bula joining afterward.

When the first testing was done with this pair in the fall (September 17), each animal maintained its former position at the grille (Bula on right and Kambi on left). This held constant throughout the remainder of the experiment. In this session four trials were completed without aid from the experimenter. Bula watched Kambi at the beginning of each trial and often during the trial. Kambi never watched Bula. The pulling was about equal (80 to 100 pounds each), and they both began their pulls at about the same time. The second test on September 23 resulted in failure after fifteen minutes, during which time each pulled alone and Bula groomed Kambi. (This was near the beginning of a dysentery epidemic in the colony, which may account for the listlessness of the animals.)

On October 14 testing was begun again, using the mechanical baiter, and with the observer concealed in the booth. The box was moved in by Bula alone on the first trial (box too light), and thereafter they failed to move the box for twenty-six minutes. At that time the experimenter came from the booth, took Kambi's rope, and helped Bula pull the box in. On the next session, number 4, they pulled together successfully for three trials, and on the fourth Kambi's motivation slackened and she did not pull any more, giving Bula no opportunity to coördinate with her. On the first three trials Bula watched Kambi and joined her in pulling whenever there was opportunity. Kambi's motivation was poor on the fifth session, and she did not respond even to additional amounts of incentive. Bula pulled hard alone and increased her activity when the extra incentive was added. The experimenter finally helped Bula by pulling on Kambi's rope. The same was true for the four trials of the next session, save the first, on which both animals worked well.

On the seventh session (October 23) Bula seemed to be very highly motivated. She watched her chances to coördinate with Kambi, and once, when she was in the back of the cage and Kambi began pulling at the front, she rushed up immediately, took her rope, and pulled in time to move the box with Kambi. At another time, when Bula was at the grille and Kambi in the rear, Bula went back and apparently touched Kambi, immediately returned to the grille, took her rope ready to pull, and then looked back at Kambi, who did not come up. A little later Bula again went to Kambi and, standing in front of her, made movements with her hands, palm up, fingers and wrist flexed, and hand moving in beckoning gesture. She also whimpered. Kambi did not come up, though Bula returned to the grille. These two acts of Bula's were the first suggestions of what later appeared as the solicitation pattern. On the next session, after five minutes spent in some individual pulling and grooming by Bula of Kambi, Bula again made the begging movements and went to the grille ready to pull. This time Kambi followed her forward, took her rope, and they pulled the box in together in four heaves. Thus, Kambi responded positively to this third solicitation.

For the next five sessions (9 to 13) the animals worked well together and pulled in the box four or five times each session in immediate response to the baiting. Bula watched Kambi and established the coördination by joining her in the pulling. Bula solicited Kambi once on the sixth trial of the ninth session after she had failed to come up to the grille for ten minutes after the box was baited. The solicitation on the fifth and seventh trials of the tenth session was done immediately when Kambi turned away from the grille after the box had been baited. When both animals began to pull as soon as the box was baited, it was usually drawn in with four or five quick heaves, in less than twenty seconds. An excerpt of the original record is given here.

Session 10, trial 5. (30″ total time) Bula is at grille. (Apparently only one piece of food delivered on Kambi's side and two on Bula's.) When Kambi did not come up at about 10″, Bula went running back to Kambi and touched her, or at least stood an instant in front of her, then returned to the grille. Kambi got up immediately and went forward to the grille where she took the left rope and held it an instant. Then they began to pull in the regular fashion, with Bula watching Kambi's movements and coördinating with them.

Trial 10. (1′ 05″ total time) Bula was up when the food was delivered and took her rope immediately, though she did not pull. Kambi did not come up for the baiting, and Bula sat at the grille looking back and forth between the food on the box and Kambi. Finally, at about 30″ or 40″, she turned and ran back to Kambi, squatted beside her for a moment, then arose (it looked as though she had touched Kambi) and ran to the grille. There she took her rope and began to watch Kambi, who was moving forward. Bula gave a sharp cry as Kambi moved forward. The ropes were crossed and Kambi was slow to take her rope. Meanwhile Bula had pulled the box once alone with both ropes. Then, as Kambi took the rope, Bula began to whimper. Both began to pull thereafter, and they got the box in with well coördinated movements.

The first trial of session 14 was typical of many in which the animals did not get started pulling directly after the box was baited, but instead engaged in uncoördinated activity for some time. Both were at the grille and watched the food delivered, and each pulled lightly on her own rope, then dropped the rope and began to play about alone, to adorn herself with the loose end of the rope, or to groom the other animal for the first eight minutes. At 8′ 50″, when they happened to be at the grille at the same time, Kambi picked up her rope and Bula immediately picked up hers, braced herself against the bars, and was ready to pull when Kambi dropped her rope. Bula let go hers and they played about, play-fought, etc., until 11′ 00″ when the same thing was repeated, except that when Kambi turned away, Bula reached out and touched her on the shoulder with the solicitation gesture. This did not recall Kambi and they both left the grille. At 20′ 00″ the experimenter left the booth and helped Bula by pulling Kambi's rope, so that they got the box within reach. On later trials Bula's solicitation became more pronounced and persistent, and served to keep Kambi pulling.

On the fifteenth session Kambi's motivation was poor, and Bula solicited only once. The experimenter came out and called the animals to work on each trial. He had to call to them once during the five trials of the next session, though they worked fairly well without solicitation on the other trials. In the next three sessions (17 to 19) both animals worked well and pulled the box in five to seven times during each session. A few instances of solicitation appeared toward the ends of the sessions when Kambi seemed to lose interest in working. An excerpt from the session 20 record is given.

Session 20, trial 1, November 18. (At 22′ more food was added to the top of the box from the baiter.) Box was baited a second time, and Bula was looking at it from the grille. Kambi was about half way back in the cage. Bula saw the food fall upon the box, watched it for three or four seconds, and then turned to look at Kambi. Kambi did not come up, so she left her rope after another look at the box and went back toward Kambi bouncing up and down on feet with arms moving. She approached Kambi and touched her with her body and then ran back and sat down at the grille. Kambi came part way forward, then turned around, and started back. Bula turned around at the grille, and holding the rope in her left hand, reached out for Kambi with her right, whimpering as she did so. Kambi turned around, came to the grille, took her position, and began to pull. Kambi did not pull very hard. Both got two pieces of food.

Trial 2. (Kambi receives no food.) The food fell off the box on Kambi's side when it came from the baiter, while two pieces were placed on Bula's side. Bula was watching the baiting of the box as she sat at the right beside the grille, and she had her hand on the rope ready to pull. Kambi was looking toward the box, though she did not come up to work, apparently seeing that there was no food on her side of the box. Bula looked around at Kambi for a few seconds, left her position and went back to Kambi, jumped up and down a little, then came up and took her rope again. Kambi did not come up, so Bula, holding her rope in her right hand, turned half way around and begged very definitely with her left hand. (She employed flexed hand, upward movements, incipient temper tantrum expression on her face and whimpering. The whimpering was much louder than on trial 1 above, consisting of a longer, lower moaning sound than had been given before.) Kambi responded to this begging, coming up to the grille and looking at Bula. Kambi took her position and took her rope, though she did not begin to pull. Bula looked around at her and whimpered, fairly loudly, and Kambi took a tighter grip on her rope. In the meantime Bula had pulled very hard on her own rope and had moved the box an inch or more. She began to whimper after she had done the heavy pulling almost alone, using the begging gesture again. This time Kambi took her rope and pulled hard enough so that she and Bula together moved the box. It came in with four or five heaves.

Although there was no food on Kambi's side, she pulled in a characteristic manner. When the box came in, Bula took both pieces slowly and held one in her hand while eating the other. Kambi's face was near Bula's mouth, though she did not try to get any of Bula's food forcibly. Kambi did not whimper. Bula apparently had no intention of giving Kambi any of the food.

The whole activity lasted about one minute. It seemed as though Bula were directing the activity throughout. Kambi certainly did not seem to want to work. It should be noted that she, before this, had very seldom worked when there was no food on her side of the box.

From sessions 10 to 20 the records showed a steady increase in the average amount of pulling done by Bula, from 100 to 150 pounds, and a decrease for Kambi, from 100 to about 50. The time relations were not always clear, though often Kambi began pulling first, lightly, and Bula joined a fraction of a second later with her strong pull, which rose rapidly to its maximum strength.

On the twenty-first session both the animals were at the grille when the box was baited, but they pulled only very lightly and did not move the box. For the next twenty-seven minutes they played quietly by themselves, climbing about or adorning themselves with the ropes. Incentive was added to the box four times, with no response by the animals save that Bula looked up briefly on each occasion. Finally, at the end of the twenty-seven minutes, the remainder of the food in the baiter was dumped upon the box, with the following result:

> (27′ 50″) Bula began walking about in back and whimpered a little. She seemed to be soliciting marching from Kambi, who responded and they marched together for one step, when Bula broke away and went to the grille and took her position at the right rope, whimpering as she went. She began to whimper more vigorously when Kambi came toward the grille. Kambi took her rope, and Bula began to pull, whimpered once, and then Kambi began to pull. They pulled together for five heaves and drew in the box.

The record showed that Kambi did practically no effective pulling; she pulled a little at the beginning (not more than 30 pounds) and then let slack accumulate in her rope as Bula moved the box alone, pulling 150 to 170 pounds. This behavior seemed to indicate that Bula was definitely aware that two ropes should always be in use when the box was to be moved, even though in this instance her rope was carrying the full load. In previous and subsequent sessions Bula was often observed to take both the ropes in hand before pulling her maximum. She sometimes pulled 100 pounds or more on a single rope when her partner was not at the grille.

Session 23, trial 1, was delayed until 2′ 30″ because Kambi remained in the back, and as she came up Bula was jerking on her own rope and looking around at Kambi. They pulled in with five heaves.. On the second trial Bula picked up her rope first and turned to Kambi, who was a little behind her, and stamped her foot with some noise. Kambi came up and they pulled in the box in 20″. Bula's action at first suggested that she was about to have a temper tantrum, which possibly may be interpreted as a form of solicitation. Four other trials were completed by prompt work by both animals, Bula watching Kambi intently. The record indicated that Kambi was beginning each heave, followed by Bula who usually finished the effective pulling alone. Kambi pulled 40 to 90 pounds and once as much as 110, though usually about 50, while Bula regularly pulled between 150 and 170 pounds.

Six trials were completed on session 23a with both animals working together well. On the second trial the food on Kambi's side of the box fell off.

> Bula came up and looked at the box, did not take her rope, but looked at Kambi and then went back to her. In a few seconds Bula came up again, played with the left rope (Kambi's), pulled fairly hard on it, then went away from grille at 1′ 00″. At 1′ 30″ Bula came back to the grille, took her own (right) rope and turned around toward Kambi, reached toward her with palm up and beckoning movements of her hand, whimpering as she did so. Food had been added to the box in the meantime, and Bula continued to hold her rope with a little pressure. Kambi had come to the grille in response to Bula's gesture, but started away again, when Bula turned half around, reached toward her again whimpering, beckoning, and bouncing up and down in a crouching position. Kambi returned to the grille, took her rope, and they pulled the box in. Bula had two pieces of food and Kambi none, even though she went to Bula and put her mouth near Bula's. (Total time—2′ 30″.)

On the next trial Kambi's food fell off again, both on the first and second baitings. Bula solicited Kambi as above, though Kambi did not respond and would not pull until the experimenter came out of the observation booth and replaced her food on the box. Another instance of solicitation appeared on the last, the sixth, trial.

On the first trial of the next session both animals came up and took their ropes when the box was baited, and then Bula whimpered just before they began to pull together. Perhaps this could be interpreted as a call to work by Bula. The first four trials were in the regular manner with one solicitation by Bula. On the fifth they did not respond immediately, playing about and getting into reversed positions at the grille. Kambi's incentive object had fallen off the box, and after the experimenter had replaced it, they took their usual positions and pulled in the box. The record showed that Kambi typically started her pulls about a second before Bula, but she usually let go before Bula had finished the heave. Kambi's pulls were sustained for 2 to 3 seconds at a constant level between 40 and 70 pounds, while Bula's pulls, which lasted between 1.5 and 2.5 seconds, rose to a sharp peak at 130 to 150 pounds.

On session 25 the first two trials were smooth and rapid, and the box was drawn in in 23 and 17 seconds. On the third Kambi's food was not delivered, but fell to the floor beside the box.

> Bula came up and took her rope ready to pull, then looked back at Kambi who had not come up. She solicited her with the begging gesture and whimpered. Kambi came up and jerked at Bula's shoulders in a playful fashion. Bula played with Kambi a little, but did not leave the grille. Bula began again to beg Kambi to work with characteristic gestures and whimpering. Bula pulled her rope once by herself, though the box did not move. She whimpered while looking at Kambi, who was sitting at the grille holding her rope but doing no pulling. Kambi and Bula then pulled together for three heaves and moved the box about half way in. Kambi dropped her rope and left the grille. Bula sat quietly, neither pulling alone nor soliciting Kambi. Then Bula took the two ropes, placed them together, and pulled for two heaves, moving the box within reach. Kambi watched as Bula took the food, but did not try to get any from her. The first pulling together was at 40″, and Bula got her food at 1′ 15″.

On the last trial the ropes were crossed twice so that each animal's rope came to the grille on the correct side. The animals were confused and did not pull, but jerked the ropes and pulled spasmodically. Later they returned to the grille and found only one cross in the ropes, so that the ends came to the grille on sides opposite from which they left the box. Each took the rope in front of her and pulled the box in. (Note that a double cross seemed to confuse the animals, while a single one did not.) Kambi pulled 50 to 60 pounds while Bula pulled about 130.

Moving pictures were taken on the next two sessions. Most of the trials were run off quickly without solicitation, though solicitation did occur on one which was photographed (see plate 1). On session 28 five trials were done rapidly, though on the sixth they seemed to have lost motivation and did not do any pulling, spending the time playing and marching. On the next session, first trial, Kambi came up to pull at 1′ 30″, and Bula took her rope ready to pull. Kambi dropped her rope and went away. They ran about and played individually until 5′ 00″, when Kambi came up again and took a rope. Bula made ready to join her in pulling, but again Kambi turned away. This time Bula solicited her, she returned, and they made one heave. Kambi went away, and the solicitation was repeated twice more, followed by one heave. Finally the box was drawn in, partly by Bula's individual effort. Two more trials were completed, though Bula turned back at the end of each heave as if afraid of something in the rear. Seven minutes of uncoördinated activity appeared on the third trial before they pulled together in response to addition of food and drew in the box.

On the thirtieth session Bula's turning behavior seemed more pronounced, as she looked back between heaves and sometimes went to the back of the cage. Kambi held her rope during these breaks in the sequence of pulling. Kambi seemed to watch Bula on some of the heaves, and once, after she had built up some tension on the rope, turned and touched Bula as if to start her to work. This is the first indication of any solicitational behavior on Kambi's part. She had been watching Bula very carefully on the third trial of this session.

During the next few sessions (31 to 36) Kambi continued to watch Bula, and it became increasingly apparent that she was trying to coordinate her pulling with Bula's, so that it might be said that Kambi had reached stage 2 in learning to coöperate. Her behavior at this stage was neither as regular nor as clear-cut as was Bula's or Bimba's at the same stage. During some of the sessions of this group, the motivation of both of the animals was poor, so that it was necessary for the experimenter to leave the observation booth and call them to work. Usually a sharp command given from the door of the booth was sufficient to get them to come up to the grille and take the ropes. The typical picture at this stage seemed to be for Kambi to build up a little tension on her rope first, then look around for Bula who would take up her rope and begin to pull. Kambi then would face forward again and pull with Bula. These impressions of time relations were corroborated on examination of the graphic records.

Sessions 36 and 37 were complete failures, since the animals did not pull together on the ropes at any time. The experimenter remained within the booth and did not come out to call them to work. They did not respond when extra food was added to the box from the baiter. On session 38, after sixteen minutes of uncoördinated activity, the experimenter came out of the booth and added grapes and orange (grapes a new incentive object in this experiment), and both animals came up immediately and pulled in the box without much, if any, watching. The thirty-ninth session was done without using any additional food, and with a great deal of watching of Bula by Kambi. Kambi pulled on each heave and seemed highly motivated, eager and interested. The graphic records showed, however, that she never pulled over 70 pounds, and the pulling she did was done jerkily, while Bula pulled from 140 to 170 pounds, in smooth heaves.

On the fortieth session Bula's solicitational behavior appeared again, and she kept Kambi at the task on two separate occasions when she had turned to go away. On one other occasion, after four minutes of play-fighting, Bula solicited Kambi to come up and work. On one of the trials of the forty-first session Bula's food fell off the box when it came from the baiter. Bula looked at the box intently for a moment and then went away. The trials of the forty-first through forty-fourth sessions were executed without any unusual occurrence, with occasional solicitation by Bula and with watching by Kambi. On the forty-fifth session, fourth trial, Bula was pulling very hard and Kambi scarcely at all, though she had the rope in hand. Bula began to whimper without turning to Kambi as she did it, although Kambi was the real source of the difficulty. On the last trial solicitation occurred in the typical pattern. This was described in some detail in the notes.

Trial 5. Before the box was baited Bula was up at the grille, and Kambi was playing with the ropes near the grille. When the food fell from the baiter both began to look intently at the box. Some of Kambi's food fell off the box. Kambi did not pick up her rope, though Bula had already picked up hers. Bula waited a few seconds, and then: Put her finger on Kambi's shoulder with no facial expression of her own. Placed her whole hand on Kambi's shoulder and turned her around toward the grille, this time with the facial expression characteristic of a temper tantrum. Kambi started toward the grille and then turned and ran to the back of the cage and climbed up on the framework. Bula pursued immediately with a whimper and caught Kambi's foot as she was climbing up. Bula jerked it hard and drew Kambi down to the floor, then began to lead her up toward the grille. After they had both gotten to the grille, Bula made gestures as Kambi seated herself and braced to pull. At the end of the second heave Kambi was not ready to pull and Bula again gestured at her (fingers flexed, palm down, arm moving up and down).

It is interesting to note that Bula does not gesture at Kambi when Kambi fails to pull hard; it is only when Kambi fails even to go through the motions of pulling. Kambi's pulling at this time was between 60 and 100 pounds.

Although Kambi was apparently not well motivated on the forty-sixth session, continued vigorous solicitation by Bula finally resulted in Kambi's working, though she never delivered over 40 pounds tension. It should be emphasized that Bula seldom undertook to pull the box entirely alone. She seemed anxious for Kambi to pull, even though it was only very lightly, and would often spend considerable time in getting her to work. Another solicitation picture is given as it was recorded on the forty-seventh session.

Trial 2. The box was baited and only Bula came up, as Kambi remained in the middle of the cage playing and making lip noises. At the first baiting Bula looked at the box for ten to fifteen seconds and then went away. (Apparently she did not think the incentive was sufficiently large to work for.) At about 40″ another incentive unit of orange and banana was added to each side. Bula was not looking when the food fell on the box, though when she looked up soon thereafter, she began to run around, passing Kambi once or twice, and then came up to her position at the right side of the grille. Kambi was still in the middle when Bula began her solicitations which lasted at least 20″, perhaps more. She first reached back with her left arm as she was sitting holding the rope with the other, touching Kambi. As Kambi did not respond, she dropped her rope and slowly moved back toward her, bouncing up and down on her flexed legs. Before she had dropped her rope she was stamping her foot rather impatiently. As she got near Kambi, she reached out with both hands to touch Kambi, though she never did take hold of her and turn her forcibly around. She did, however, hook her hand under Kambi's armpit and turn her about a little. Bula was whimpering all the time that she was with Kambi. Kambi came directly to the grille when she left her position in the middle of the cage. As soon as Kambi came up, Bula was in position at the right side of the grille and was ready to pull. On the first three heaves Kambi held her rope and moved it only a very little, hardly taking up the slack. At the end of the third heave Kambi dropped her rope and began to turn away. Bula solicited her with gestures until she took the rope again. They pulled together on the last two heaves. (Total time—2′.)

Solicitation occurred on all sessions from the forty-seventh through the fifty-first. Kambi's motivation was very poor, and it was often only after solicitation that she pulled at all. On session 52 Bula did no soliciting and they failed to draw in the box at all during the allotted twenty minutes. During the last three sessions in this group they were also poorly motivated. They played vigorously with each other and ran about the cage, but worked at the ropes only when the experimenter called to them. After the fifty-fifth session experimentation was discontinued for ten days.

After the interim of rest, the animals still did not seem interested in the problem, although on the fifty-seventh session they pulled together after Bula's solicitation. Immediately following this session the pairings were changed in order to see if Bula would solicit Alpha as she had solicited Kambi and Bimba. Two more sessions, 58 and 59, were given after training with Alpha and Bimba in order to see if Bula would still solicit Kambi. This she did vigorously four times during the two sessions.

C. Bula and Bimba

Bimba and Bula were used together for six sessions during the spring of 1935. At no time during this experimentation did the animals work together coöperatively. On all but the first session, the experimenter took an active part in the situation, helping pull or push the box, repeatedly presenting the ropes, and calling to the animals. Bula responded to the experimenter's activity, and pulled with him or came forward when he called. Bimba, however, only once joined either the experimenter or Bula in the pulling. Often the experimenter and Bula pulled the box almost all the way in together, and then the experimenter gave his rope to Bimba. On only one occasion did she take the rope and pull. Bula joined her and they moved the box within reach. Bimba did a good deal of pulling by herself when Bula was not at the grille, and occasionally when Bula was there. While Bula often watched Bimba, as she had learned to watch Ross (see the account of the training of Ross and Bula), she did not use the opportunities presented to join Bimba in the pulling. Bula either remained at the grille and passively watched Bimba pull, or went away from the grille when Bimba came up.

This pair was used for the first time in the fall experimentation on September 16. The behavior during this session was typical of that described for the spring sessions. Bula once took the rope out of Bimba's hands and, placing the two ropes together, pulled on both at the same time. During this session Bula and Bimba came up to the grille on sides opposite to those they habitually used during the previous sessions. They seemed confused for a few moments; then Bula touched Bimba on the arm and they immediately changed positions. This small gesture seemed to be understood immediately by Bimba—as if it were a signal to do something already planned for.

The pair was not used again until November 6. In the meantime Bula had been used with Kambi for seven sessions, and with Alpha for five. Bimba had not been used with any other animal during the interim. On sessions 2 and 3 no coördinate pulling appeared, and the experimenter remained outside the experimental room in the observation booth for the first twenty minutes. At the end of the second session he went outside, pointed to the ropes, and tried to induce the subjects to take one, but he was unsuccessful. The uncoördinated activity was characterized by Bimba's jerky, short pulls, hardly long enough to give Bula an opportunity to join with her. Occasionally Bula did attempt to join Bimba in the pulling. The following is a typical description of such an attempt:

> Session 3 (14′ 20″). After much playing, Bimba settled down on the left side of the grille with her rope in hand and braced her feet against the bars as if ready to pull. Bula took her position on the right beside her, watching Bimba. She waited thus for four or five seconds while Bimba held her rope. Bimba then began to play with her rope, and Bula let go hers and turned away to play in the rear of the cage.

Session 4 was likewise uncoördinated, save for one attempt by Bula to join Bimba in the pulling, which failed in the manner indicated above. After twenty-two minutes, the experimenter left the observation booth, went to the cage grille, and presented both the ropes. Bula took hers and braced herself ready to pull, though Bimba would not take hers. The experimenter pulled once on Bimba's rope and was joined in this pulling by Bula, so that the box moved a little. He then handed the rope to Bimba who took it immediately, braced herself, and began to pull. Bula followed her movements, and they drew in the box together in a succession of heaves. This was the first coördinated pulling done by this pair.

The next six sessions (5 to 10) included from three to six trials each. On almost every one of these trials there was solicitation of Bimba by Bula. We quote a few typical descriptions of the acts of solicitation.

> Session 5, trial 2. Bula was sitting in her position on the right side of the grille when the box was baited. Bimba was sitting in her position, though she did not begin to pull on her rope. Instead, she started to climb toward the top of the cage. Bula had been watching Bimba during this time, and as Bimba did not make any move to pull the rope in twelve seconds, Bula began to bounce up and down on flexed legs, reaching out toward Bimba's head. She seemed to be pulling Bimba down to the floor and the ropes. Bimba took her rope and they began to pull together, getting in the box within thirty seconds.
>
> Trial 3. Both animals were sitting beside grille, Bula in position. Before fifteen seconds, during which time Bimba did not work, Bula had reached around to Bimba, holding her rope with her right hand and touching Bimba with her left, and drew her around until she took her rope and began to pull. Bula's typical gesture here, as on other trials, consisted in the inverted palm, holding it over the upper part of Bimba's body near her shoulder, moving it up and down. Bula was in the squat position, bouncing up and down on her legs. Occasionally she whimpered as if she were beginning to have a temper tantrum.
>
> Session 6, trial 4. Bula at the grille and Bimba near, but not holding the rope. Bula turned to Bimba and gestured with a flexed forearm, palm down, with her mouth wide open and making a sound something like whimpering, but with very little vocal cord activity, perhaps characterized by the consonants, "Ghk, G-h-k." Bimba took her rope after this solicitation, and they pulled the box in.

During these sessions the pulling was not distributed equally. Bula pulled between 100 and 150 pounds, while Bimba seldom delivered over 60–70.

After the twelfth session (the eleventh spent on Problem IV, see table 1b), solicitation by Bula did not occur, probably because Bimba was watching and following Bula in her pulling. At the same time, on a number of trials, Bimba seemed to be going ahead alone, and Bula followed her. Both animals apparently understood the situation, and either might act as leader or follower. It is justifiable to say that Bimba had reached the second stage in the mastery of the coöperative habit; she had learned to watch her partner and to join her pulling. On the sessions following, through the twenty-fifth, the behavior of Bimba gave increasing indication of her awareness of the function of her partner. On some of these sessions (13 to 16) Bula seemed to be afraid of something and turned away from the grille after each heave. Bimba watched Bula closely during these retirements, and waited for her to come back, beginning to pull when Bula was ready to join her. Bula continued to do the maximal share of the work, pulling 150 pounds when Bimba seldom pulled more than 50. On the second trial of the nineteenth session Bimba picked up Bula's rope when Bula had dropped it, turned away after a heave, and shook it. It was suggested that perhaps Bimba was attempting to attract Bula's attention to the work by this move. Sessions 20–25 were typified by four or five smooth rapid trials, during which each animal pulled rhythmically and without much watching of the other. This was the sort of coöperation which appeared when each of the animals was well motivated and went directly to its task. It was, from all points of view, the most efficient.

On the first two trials of the twenty-sixth session, Bimba watched Bula very intently during the times when Bula was holding her rope but was not pulling. On the third trial Bimba made her first solicitation gesture during the experiment. Its appearance was described in the notes, following the dictaphone record, as follows:

> Trial 3. (Dictaphone observation recordings began at 3′ 30″.) Both took their ropes immediately when the box was baited with two units of food, and pulled twice, moving the box about one foot. For the three minutes during which they did not pull, Bimba had been at the grille looking often at Bula and holding her own rope part of the time. Bula played about and did not attend to Bimba. Bimba looked at Bula for a few seconds, and it appeared that Bula was about to pick up her rope, but she did not, and turned away. Immediately Bimba put out her hand and placed it around Bula's neck as if she were trying to move Bula back to work. Bula did not respond, and they both went toward the back of the cage. Bula then returned to the grille, and Bimba came up and took her position beside her. They took the ropes, but did not do any real pulling. They began to work and made two heaves with Bimba watching Bula. After the second heave Bimba was holding her rope and Bula started to go away, though seemed to hesitate a little. Bimba put out her right arm, holding her rope with her left, and put her hand around the back of Bula's neck, touching it lightly. Bula turned back to the grille, picked up her rope, and they pulled together.
>
> *Note.* In this gesture Bimba touched Bula on the shoulder and behind the neck lightly, moving her hand up and down a little. She did this without any apparent emotional expression, very "matter-of-factly," in contrast to Bula's vigorous and emotional behavior during solicitation.

Bimba continued to do a very small amount of pulling, even after she began the solicitational behavior. She continued the solicitation of

Bula on almost every trial until the thirty-sixth, when the testing on Problem I was discontinued.

On the twenty-eighth session, second trial, Bula seemed to be annoyed by Bimba's solicitation. This was an unusual occurrence. Usually the animal solicited either responded fairly promptly and began to pull, or definitely refused and went to another part of the cage. On this occasion Bula seemed to object and yet to comply. The notes on this trial are quoted below, in order to indicate this objection by Bula and to illustrate how completely Bimba dominated and directed the activity.

Trial 2. Box baited with one unit of food. Both came up to the grille, but sat down in reverse positions. Bimba touched Bula and she moved out of the way, and sat down at her accustomed place on the left side. As soon as she was in position, Bimba put out her hand toward Bula and touched her on the neck, trying to get her to begin pulling. Bimba kept her hand there a number of seconds. Bula apparently did not want to work, and Bimba had to turn and solicit her three times before she began pulling, and then solicit her on each heave. On the second heave, after three separate solicitation gestures by Bimba, Bula made a wry face, began to whimper, but began also to pull. While Bula was pulling the rope, Bimba left her hand on Bula's neck and pulled herself with only her left hand, and hence not very strongly. Total time, 1′ 30″.

On the twenty-ninth session, after the ten-day rest, neither animal responded to the first baiting of the box, but play-fought, played with parts of the cage, etc., for 10′ 20″. On the second delivery of food by the baiter Bimba began to solicit Bula immediately, and kept at it until they had moved in the box. In many trials Bimba kept her hand on Bula's neck and patted it between pulls as if to keep Bula from turning away.

Bimba continued to solicit on each of the remaining sessions, and the box was pulled in four or five times on each session. Bimba went about her solicitation in a calm and determined manner, without excitement, but with persistence which usually resulted in Bula's pulling. Inasmuch as her solicitation was so persistent and usually successful, and since she did only a small share of the actual heavy pullling, it may be said that she seemed to dominate the situation and exploit the aid of her partner to the fullest.

A few excerpts from the records will serve to give a more complete picture of Bimba's solicitational behavior.

Session 29. (Neither animal had been doing any pulling. They had been playing about until incentive was added to the box at 10′ 20″.) *Food added.*

Immediately Bimba put her hand on Bula's neck and they pulled together once, revealing three units of food on each side of the box. Bula dropped her rope at the end of the first heave, and Bimba put her hand on Bula's neck; whereupon Bula returned to the grille. They pulled together on the second and third heaves, after which Bula dropped her rope and started away again. Bimba held her rope for a few seconds while Bula went to the back, and then she dropped her rope and looked a few seconds at Bula. Bimba reached out toward Bula and moved her arm in a beckoning gesture. Bimba seemed to hoot very softly once or twice during this gesture. Bula came up, they pulled the box in, and each took food.

Session 33, trial 3. When box was baited, Bimba was in the back and Bula near the front. Bimba started for the front immediately and caught hold of Bula on the way up, and, putting her arm around her, brought her along to the grille. They both sat down and Bimba took her rope, then touched Bula to get her started. Bimba had to solicit Bula on every heave to keep her at the task, as Bula appeared very reluctant to pull. The box was drawn in within 60″.

Note. Bimba characteristically solicits Bula (by touching her neck or shoulders) to pull, using one hand while the other is on her rope. She then puts both hands on her rope, turns face forward, and begins to pull. If Bula does not begin to pull, Bimba lets go her rope with one hand and reaches out for Bula again, keeping at it until Bula begins to pull, or at least is braced ready to pull. Bula, on the other hand, often responds to solicitation by taking her position at the grille with rope in hands and feet braced, and then watching Bimba until Bimba starts to pull, at which instant (or a second later, as shown by the graphic records) she begins to pull.

D. Bimba and Kambi

Testing was begun with this pair on October 30, 1935. Previous to this date Bimba had been used with Bula once in Problem I and six times with Alpha in Problem IV. Kambi had been used with Bula thirteen times in Problem I (with a few short solicitations by Bula and no clear-cut response by Kambi), and during the previous spring, fifteen times (unsuccessfully) with Alpha. The first sequence of testing consisted in seven sessions running from October 30 to November 5.

In no session of the total twenty-eight did the animals ever pull coöperatively to draw in the box, nor did they ever obtain it by individual pulling. On the first seven sessions, the experimenter remained within the booth for almost the entire twenty minutes of the session. At the end he usually came out and called to the animals or presented the ropes in an attempt to get them to work. On the first session Kambi spent the first few minutes at the grille pulling spasmodically at the rope fairly hard, while Bimba played with the ropes at the grille and ran about the cage as if she had no notion or intention of pulling at all. On the later sessions this picture was typical. The animals engaged in very little play together, such as tag, play fighting, tandem marching, or grooming. They actually seemed to avoid one another, particularly at the grille. It was almost a regular occurrence that when one came to the grille, the other left. Usually Kambi occupied herself in the rear of the cage, and Bimba did her playing in the forward part at the grille, adorning herself with the ropes, biting at the ends, or playing with the grille bars. Bimba gave the observer the impression of having a thoroughly good time. Kambi came up to pull occasionally and then retired to her corner in the rear. Addition of incentive by the baiter had no effect on the activity.

The second group of sessions was begun on November 18, and lasted through the 29th. In the meantime Bimba had worked with Bula for eight sessions on Problem I (responding to Bula's solicitation), and four with Alpha on Problem IV. Kambi had worked ten sessions with Bula on Problem I, and had responded to Bula's solicitations there. Kambi and Alpha had worked at Problem I for three sessions unsuccessfully.

On sessions 8, 9, and 10 the technique was the same as above; the experimenter stayed within the booth for the full twenty minutes. The animals again failed to pull together, though they did more individual pulling. They engaged in a great deal more joint activity, play-fighting, chasing each other about the cage, and Kambi groomed Bimba. On the eleventh, twelfth, and thirteenth sessions the experimenter remained outside, baited the box manually, presented the ropes, and bent over the box to help in the pulling. Once more the animals failed to pull coöperatively.

The next group of sessions began on January 6, 1936. Previous to this time Bimba had learned to solicit Bula in Problem I. We wanted to discover whether she would solicit Kambi even though she had never pulled coöperatively with her. This she did on four occasions, two of these consisting in Bimba's going to the back of the cage, putting her arm around Kambi, and bringing her all the way to the grille. On the other two occasions she remained near the grille and persisted in reaching for Kambi's neck until Kambi shook off her hand.

Session 17 (6′ 40″). The switch was clicked and one unit of food added by the baiter. Bimba, who was rolling around on the left side, looked up and watched the food fall upon the box. She then got up and went immediately to Kambi in the rear of the cage, put her arm around Kambi's shoulders, and started forward with her. Kambi bent down and seemed to shake off Bimba's arm. Bimba withdrew her arm, rolled over, and began to play alone.

Session 19 (9′ 30″). The experimenter went out and moved the baiter back, exposing the food on the box to clearer view. He returned to the observation booth at 10′ 10″. Both animals had come up to the grille, finding the ropes crossed.

Bimba took the right rope at the left grille, and as Kambi came up she put out her hand and placed it on Kambi's neck, moving it up and down in her characteristic solicitation gesture. Kambi started to take a rope (the left one), whereupon they became confused, and Kambi dropped her rope. Bimba dropped her right rope without any more solicitation.

On none of the eight sessions did the animals pull in the box together. However, the solicitation pattern of Bimba was so clear and so similar to that shown with Bula (described in detail in the Bula-Bimba summary) that it left no doubt that Bimba had learned the solicitational behavior as a generalized method of problem solving. The last two sessions, 22 and 23, also resulted in failure.

E. Supplementary Information from the Graphic Records of Pulling

In this section are presented certain general observations on the characteristic manner in which each animal pulled its rope, both alone and in the social situation. References to the original graphic records have already been made in the summaries above. The interest in the relative amounts of work done by coöperating animals was the chief reason for the use of the recording mechanism, and there its most important contribution to the data as a whole was made. The records also were used to study time relations of pulling, especially to discover whether, of two animals working together, one would always begin pulling first. Often, when watching behavior occurred (stage 2), gross observation failed to reveal whether the watcher delayed her pulling until after the partner had started. In some of these cases graphic records revealed a small but consistent difference in the time at which each began to pull, while with other pairs no consistent difference was noted. When these differences in timing seemed significant, they were mentioned in the course of the summaries. The tracings have also revealed characteristic differences in each subject's pattern of pulling, which will be described briefly below.

In figure 2, tracings of typical stylograph records for each pair of animals used in the fall and winter experimentation are represented, except for Bimba and Kambi, who never successfully pulled together. The two curves for each member of a pair are drawn one directly underneath the other, so that the same instant of time is represented at any point on a given ordinate. The records are to be read from left to right. The scales for amount of pulling in pounds are shown for each row of curves on the left of the figures. It should be noted that the top scale for each pair of curves is shorter than the bottom one, because the two spring balances in the recording unit had scales of different lengths. The upper curve (shorter scale) is for the right hand balance and rope, and the lower (longer scale) is for the left hand ones. This difference in scale lengths should be constantly borne in mind when making comparisons between curves.

At the top of the figures, in samples 1, 2, 6, and 7, tracings of the records made by each animal working alone are presented. These records were made at the end of the experiment (February 25, 1936) with the same weight in the box for all subjects. Each animal used the rope on her preferred side, that is, the one customarily used when paired with another animal. Right-hand rope records are presented for Bula and Alpha, and left-hand ones for Kambi and Bimba, with the scales drawn accordingly. Alpha was not always consistent in her side habit when paired with various partners.

The sample records presented for each pair are divided into two parts: the one, marked "a," represents a sample taken early in training, and the other, marked "b," one taken late in training. Except for the records of Alpha and Kambi, and Alpha and Bimba, each sample represents a complete pull-in of the box, or trial, made up of four or five separate heaves, or effective joint pulls. The recording paper was sometimes stopped between heaves, when the animals had gone away from the grille, and started when they came back to take the ropes again. Sample 10 is presented to show in detail how the records may be read. Here movement of the box is indicated by breaks in the straight line at the top. (This line is omitted in other samples.) By erecting ordinates at proper points it may be seen that the box began to move whenever the sum of the two pulling curves equaled the tension necessary to overcome the inertia of the box, which was, in this case, between 130 and 140 pounds. The coefficient of sliding friction of the box against the track (approximately 0.45) was large enough so that about the same amount of tension was required to keep the box moving as to start it. Hence, records of slow pulling are flat-topped. In sample 10 one instance is shown in which the pulls of the two partners failed to overlap enough to move the box.

The general patterns of pulling, as represented by the records, showed interesting consistencies for each animal. Examination of the tracings reveals certain characteristics in each animal's pulling which recur frequently. Very often a record could be identified by its peculiar

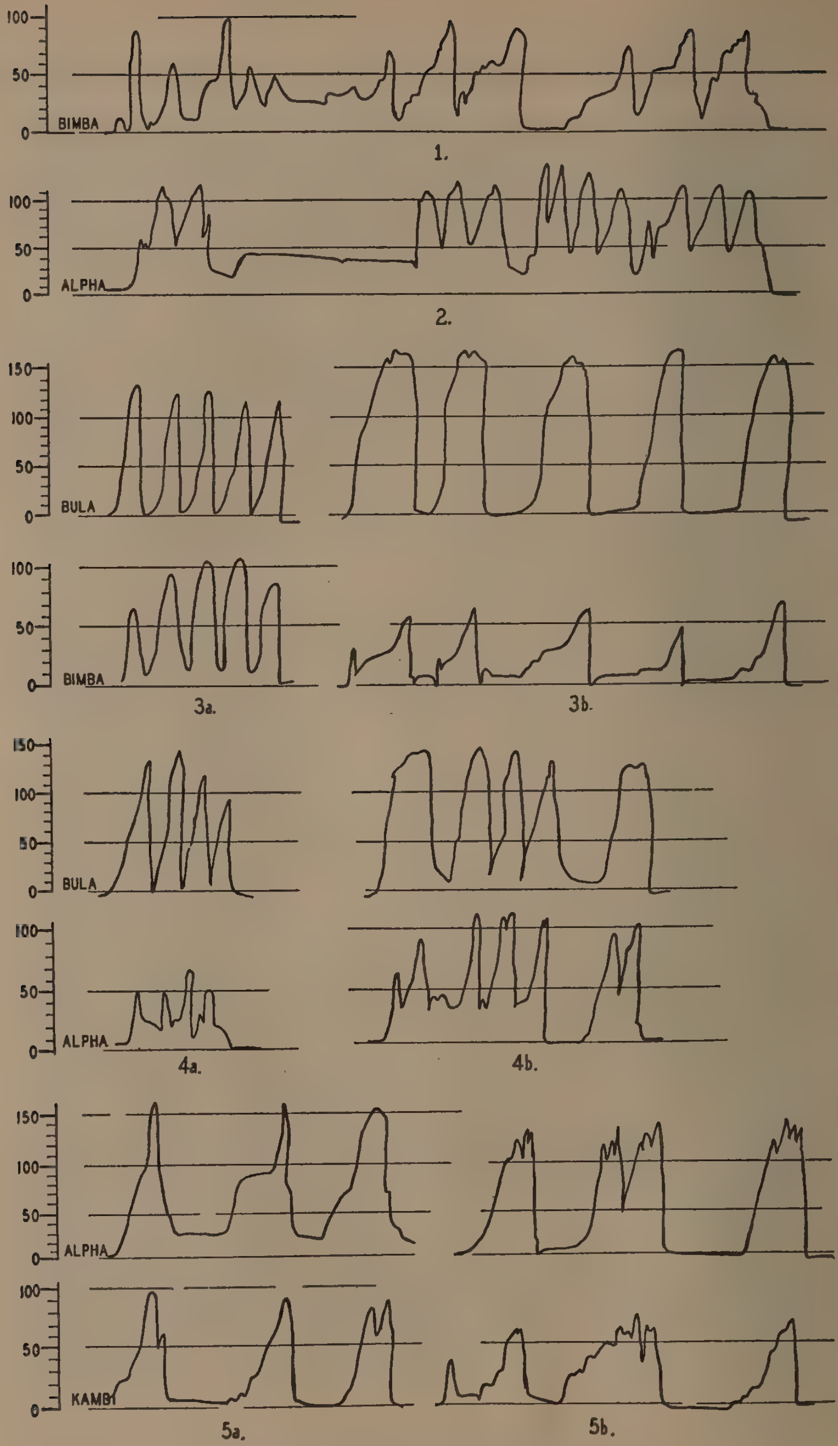
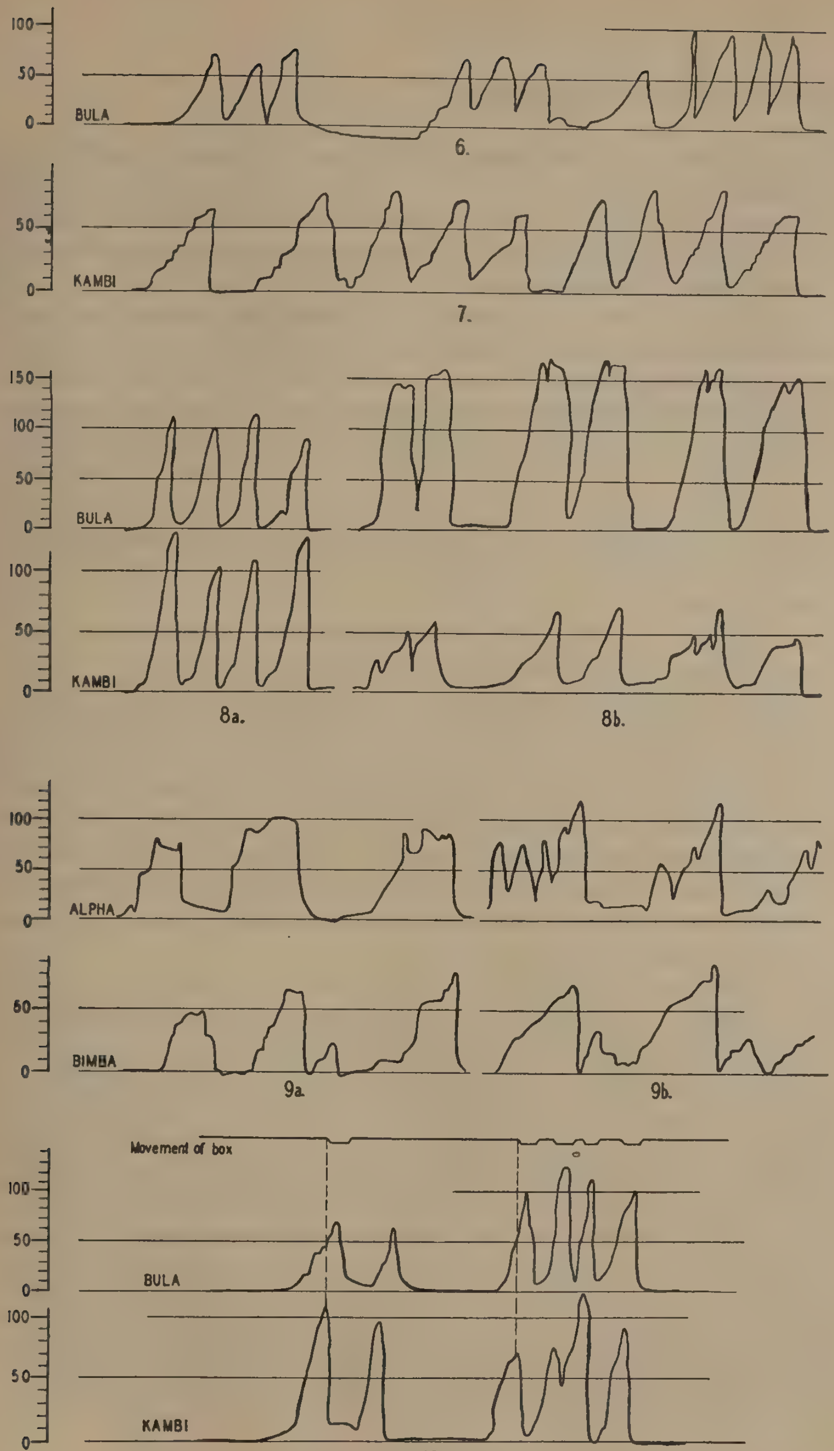


Fig. 2. Samples of Graphic Records of Pulling
For description see text



6.

7.

8a.

8b.

9a.

9b.

10.

Fig. 2—*Continued*

shape. Certain of the recurrent patterns in each animal's record are as follows:

Bimba: Slow rise in tension, often in small steps; maximum tension held for only an instant; rapid fall, as if the rope were released and the spring allowed to recoil immediately.

Alpha: Rapid rise to a sharp-peaked maximum; sloping fall, often not returning to the base line, but followed by a sharp rise before all tension in the spring was released. Sometimes a record for a complete trial consisted in a series of peaks, and would not touch the base line until the end, indicating that she kept some tension on the rope throughout the trial.

Bula: Rapid rise in tension; records tended to be round or flat-topped, indicating that she often pulled maximally for more than an instant; fall in pressure almost perpendicular, as if the ropes were completely released.

Kambi: Slow rise in tension, smooth or stepwise; peaks rather sharp; sudden drops usually falling completely back to the base line.

While all the above characteristics were not present in every record for a particular animal, enough of them would usually occur to make the pattern recognizable, regardless of the amount of tension built up on any particular occasion. Gross observation of pulling behavior tended to substantiate the differences between animals which were indicated by the graphic records. Unfortunately, no individual records were obtained before experimentation began, so the changes which probably took place in characteristics of pulling with individual and social training cannot be observed.

The change which took place in the relative amount of work done by each partner of a pair as training progressed was very striking in the partnerships of which Bula was a member. These changes are clear in the samples for Bula and Bimba, and Bula and Kambi (samples 3 and 8). In the early trials of each pair Bula seldom pulled over 100 to 110 pounds, while later she was pulling 150 or more. Her partners started at about 100 also, but dropped to 50 as Bula increased. Just the reverse was true in the Bula-Alpha combination, where the partners started unequally and gradually approached the same load.

Certain relationships between the patterns of graphic records and types of behavior observed should be mentioned. Bimba was soliciting Bula during the trial represented in the late sample for Bula and Bimba (sample 3b). Bimba would take her rope and hold it at about 10 pounds tension while soliciting Bula with the other hand (stage 3 in learning to coöperate). These periods are indicated by plateaus on the tracing for Bimba. Bula would then pull maximally in response to solicitation and Bimba would join her near the end of Bula's pull. In the Bula-Kambi record (sample 8b) Bula solicited Kambi, then started pulling heavily herself, after which Kambi joined the heave. The Kambi-Alpha samples (5a, 5b) show that Kambi's pulls began slightly later than Alpha's. Kambi usually watched Alpha and followed her pulling (typical of stage 2). The records for Alpha and Bimba (9a, 9b) and the early sample for Alpha and Kambi (5a) were made when the animals were responding to a common cue given by the experimenter (stage 1). It is seen that even under these circumstances the animals did not begin their pulling at exactly the same instant.

VI. STAGES IN LEARNING TO COÖPERATE: A GENERAL SURVEY OF THE RESULTS

A. Initial attack on the problem

Examination of the summaries for Ross and Bula (pp. 20–28) indicates how completely lacking any ability for coöperation, of the type required by the experiment, seemed to be in these animals when they were first placed in the two-rope, heavy-box situation. Each was familiar with the box pulling, and had demonstrated previously that it was capable of pulling more than half the load of the heavy box.[7] Since there was nothing in the preliminary training to suggest the use of two ropes (in contrast with the preliminary individual training for Problems II and III), we have supposed that the subjects began work on this coöperative problem "tabula rasa." It is true that Kambi and Alpha had finished fifteen sessions on Problem III and three on Problem II before beginning Problem I. But, since their behavior was substantially like that of Ross and Bula, no transfer of a general principle of coöperation or notion of dependence on the other animal is indicated, (see pp. 82–83 for a discussion of transfer of training).

To the naïve observer the two-rope situation as presented might appear to offer clear opportunity for the display of *insightful* behavior. A fundamental requirement for this (Köhler, 1925) is that all parts of the problem must be within view of the animal simultaneously. Such a situation obtained here, since ropes, box, and other animal were all clearly visible to both subjects. No member of the original pairs of animals tested displayed behavior remotely suggesting the required insight. The initial behavior represented an attack almost opposite to simultaneous pulling.

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[7] While no measurement of the factor of *social facilitation* in pulling was made, the fact that larger loads were pulled by the single animal, when in the presence of another than when working alone, indicates its positive influence.

A comment on the nature of the initial behavior should be made. The animals characteristically worked *one at a time* at the pulling. They might sit at the grille and pull alternately, each on its own rope, while the other watched. They might work alternately at the same rope, passing it between them when one was tired of pulling. Of course they did not work steadily at the problem, but interspersed intervals of pulling with play of various sorts: with each other, with the rope ends, or with parts of the cage. This type of attack is typical of the behavior observed in the living quarters when two animals discover a new object in their cage. They often sit together before the strange object, but ordinarily they examine and manipulate it alternately.

During the experiment an observation was made which well illustrates this sort of behavior. A part of the hardware cloth of the experiment cage became detached from its wooden framework. The loose end soon attracted the attention of both Bimba and Bula, who immediately attacked the hanging wire and attempted to pull it further from the frame. They sat beside the cage wall, each within easy reach of the loose end, and fingered the projecting part alternately, tugging at the wire. Had they grasped the wire together they might easily have torn it from its support. Alone, they only loosened it. As long as the wire remained loose, this was their characteristic attack on the interesting object. This and other observations recorded in the laboratory notes indicate that *alternate individual manipulation* is a characteristic and natural way for chimpanzees to attack a strange object which is equally available to two of them at the same time.

In the light of these comments it may not be assumed that the original pairs of animals would have chanced upon simultaneous and coördinated pulling as a means of drawing in the heavy box. Perhaps this might have happened fortuitously had they been allowed to continue their natural attack indefinitely. As this did not seem likely, steps were taken to *train* the animals in the preliminary requirement for this kind of coöperation, *simultaneous activity*.

B. Training in simultaneous activity

In order to stimulate the animals to work simultaneously at a task, two boxes were presented in place of one. Each box was lightly weighted and could be drawn in easily by a single animal. They were placed side by side with a rope leading from each to the cage grille. Equal amounts of food were placed on each box. Trials with two boxes were interspersed among those with one, and were arranged in a manner to indicate immediately any transfer of training from the two-box to the one-box situation. This training was given to each of the two original pairs, Bula and Ross, and Kambi and Alpha.

At first, even with the two boxes, the animals did not pull simultaneously. One usually sat and watched the other do all the pulling, although he or she might try to get some of the food when one of the boxes came within reach. After two or three sessions, the members of both pairs began to pull rapidly as soon as the boxes were baited. When this simultaneous activity was established the single box was again introduced. The records indicate that there seemed to be an immediate effect of the two-box training. The rapid and independent pulling which each animal did on the single box rope was also done on one of the two ropes of the common box. Each had learned to take a rope and pull vigorously on it. The efforts of the two subjects sometimes overlapped, moving the box a little, so that by continued effort of this sort they finally obtained the lure. The animals pulled without any apparent attention to one another's pulling, and thus a sort of "pseudo-coördination" was built up. This lasted for some trials with Ross and Bula, although not long with Kambi and Alpha, who were not continued after the fifteenth session during this period of testing. Eventually it broke down completely.

C. Stage 1. Responding to a common external cue

It was apparent that even the training in simultaneous activity (at least activity that occurred at *about* the same time) was not sufficient for the requirements of the experiment. Something more was necessary: a cue whereby the animals could time their pulling exactly together. In the presentation of this external cue, and in the development of the animals' response to it, there occurred what we have described as the first stage of co-operative behavior. In the summary record for Bula and Ross, pp. 22–25, is given a detailed account of this training. It may be adequately summarized here by the indication of certain steps taken with each pair. These steps, as enumerated below, consisted either in the elimination of certain stimuli from, or the addition of new stimuli to, a complex to which the animals seemed to be responding.

1. The experimenter helped the animals by pulling or pushing the box from front or rear. This drew the attention of the animals to the box and made the task easier for them when they pulled.
2. The experimenter accompanied his efforts with the vocal call "pull." He gave this call just before the animals began their pull on each heave.
3. The experimenter did less and less actual work on the box while, in apparent response to his call, the animals did more and more.
4. The experimenter did no pushing or pulling at all, but simply touched the box and called. The animals did all the work, although they still required a visual (experimenter bending over the box) as well as an auditory cue to get them started.
5. The experimenter stood away from the box and track and called to the animals, who responded simultaneously, moving the box with a series of smooth, regular heaves.

When the fifth step was reached, the subjects seemed dependent on the auditory external cue only to get them started. Thereafter they usually were able to pull in their own rhythm[8] and to draw the box all the way in with four or five heaves. This training procedure was followed with Ross and Bula, until each seemed proficient in responding to the auditory cue. With Kambi and Alpha it was also followed, although not with the same regularity as with the first pair. The steps ensued in the same order, although Alpha's characteristic fear of being left without the experimenter's presence in the experiment room markedly complicated the picture. She practically refused to work when he was in the observation booth. Since with Kambi and Alpha we were trying to discover whether the training sequence could be stopped at any point and yet have the animals continue to develop alone, the experimenter retired often to the booth, which was not the case with Ross and Bula. At the end of training both Kambi and Alpha responded readily to the call of the experimenter.

A partial sequence of the training for this initial step was undertaken with Alpha and Bula on the first sessions of their pairing. Alpha seemed to respond fairly readily to the experimenter at that time, as did Bula. A few attempts were made during the spring experimentation to use this training with Bimba and Bula. No progress was made by Bimba during the six sessions which are reported in the summary record for Bimba and Bula, pp. 37–38.

D. *Stage 2. Watching and responding to partner's pulling*

In the discussion so far we have traced the development of behavior in each of the two original pairs. Since the four animals were paired in only two combinations, a description of the activity of each pair has constituted an adequate account of each individual's behavior. Also, behavior characteristic of stage 1 had to be performed by each animal in the same manner (both animals responding alike to a common stimulus). In the subsequent stages each partner played a different rôle. In fact, the type of coördination there actually depended on the *difference* in the rôles assumed by each animal. Therefore, we shall hereafter consider changes in the behavior of individual animals as they worked with various partners. Each of the four subjects was paired in all six possible combinations. It was even found that certain animals played different rôles when paired with different partners.

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[8] The nature of the two-rope-box mechanism facilitated rhythmic pulling. When an animal began to pull, the box did not move until the partner began pulling also. Thus, movement of the two ropes and the box began at the same instant. If each started its pulling with outstretched arms they both would finish the pulling stroke at about the same instant. This gave a basis for the rhythm in pulling. The break in rhythm usually occurred between strokes, when the animals were taking up slack and bracing themselves for another heave. During this interval they seemed most distractable.

The distinguishing characteristic of stage 2 was the watching of the partner and the responding to his or her activity. When an animal had been observed to do this consistently for a number of trials, it was indicated that he or she had arrived at stage 2. Bula was the first animal to exhibit watching. Hence her behavior, as she worked with Ross, afforded the only developmental picture which might not have been distorted by observation of the watching behavior of another animal. We may consider the account given for Bula and Ross as typical of the way in which watching behavior developed. It should be noted that Bula had been paired with all the other animals before they first exhibited watching.

As was indicated in the last section, Bula had at first learned to respond to a complex of stimuli. When it was found that the verbal call could be omitted, it was discovered also that she seemed to be responding to another stimulus from the complex: namely, the tightening of Ross' rope against the box. (It was usual for Ross to respond first to the verbal cue and to begin work earlier.) This tightening was regularly accompanied by a squeak of the rope. Bula was observed to turn from whatever she might be looking at, take her rope, and begin to pull after one squeak from Ross' rope. The sound probably served as an effective cue. Thus the activity of the partner began to afford a cue to Bula as a substitute for the behavior of the experimenter. Gradually Bula became more responsive to the behavior of Ross himself. As training continued, Bula responded to a greater number of the details of Ross' behavior, and to behavior which preceded pulling by longer intervals of time.

It should be noted that during this progressive change in Bula's behavior Ross continued to respond to the baiting of the box by pulling fairly regularly. It is obvious that coördination of acts or coöperation at this stage depended on the immediate response, by one partner, to the placement of food on the box. If this partner did not respond immediately to this external or non-social stimulus, the other partner was left without cues to which he might respond, or, more simply, with no pulling to which he might add his own efforts.

Bula's watching behavior and her immediate response to the activity of her partner appeared not only with Ross, but with all the animals with whom she was paired. After she had learned to watch Ross, she watched other partners, such as Bimba, particularly at the beginning of the sessions on which they were paired. The extent to which she watched subsequently depended on how much opportunity the partner afforded for coördination. If the partner pulled often, Bula watched and improved her opportunities to coördinate. If her cagemate pulled little, Bula soon learned to pay scant attention to it.

The development of stage 2 with the other three animals seemed to follow roughly the course outlined for Bula. With Kambi the first clear indications of watching came when she was paired with Bula. They were also unmistakably observed later when she was working with Alpha. Kambi never seemed to watch Bimba in this manner, probably because Bimba did so little pulling. Kambi's first watching behavior with Bula appeared when the latter's interest seemed to lag and she did not solicit Kambi, as she had been doing, and did not pull alone. Kambi had been watching the box and ropes very carefully when pulling. Then, when Bula did not behave in the customary fashion, Kambi looked directly at her.

The development of Bimba's watching seemed to follow a similar course. She had been pulling with Bula, in response to Bula's solicitation as well as voluntarily. Bula's motivation seemed to lag, as it did when working with Kambi, and Bimba was observed to look around at her. From examination of the original records it appears that both Kambi and Bimba had occasionally watched Bula even when both the partners were pulling well. The lags in Bula's interest simply furnished the first opportunities for clear-cut observation of isolated acts of watching. Neither Bimba and Alpha nor Bimba and Kambi were successful in drawing in the box without aid from the experimenter. Little watching was observed with these pairs.

Watching by Alpha developed only in the company of Kambi, and during the course of learning to respond to the cues presented by the experimenter. Although Alpha had been paired previously with Bula and Bimba, both of whom had displayed the watching behavior, she showed none when intensive training began with Kambi. Therefore, the development of Alpha's watching may be fitted into the shift-of-cues pattern presented for Bula above. Alpha never became entirely independent of the experimenter. She seemed to require his presence in front of the experiment cage, and often his activity, to get her started pulling. While she responded to his call at the start, she seemed, during most trials, to shift her attention to Kambi and to the movements of Kambi's rope for the cues necessary for coördination, so that she was responding to Kambi's behavior during the major part of the pulling-in trials.

It should be noted that as far as we have been able to discover, *imitation*, (see Warden and Jackson, 1935) played no clearly defined part in the learning of watching behavior. No sudden appearance of watching of the partner herself, not preceded by watching of the experimenter, the box, the ropes, etc., was observed. However, the possibility for the occurrence of imitation is obvious, and it may have operated in a way not detectable in our results. All that we can say at present is that the developmental sequence outlined above seems to describe the appearance of watching behavior in all the animals satisfactorily.

E. Stage 3. Soliciting the partner to pull

After both Bimba and Bula had learned to watch any animal with whom they were paired, and to join his or her pulling at the appropriate instant, a further development took place in the behavior of each. While it occurred at different times, and under somewhat different circumstances, the net result was the same in each case. Each animal, when her partner did not make any movements preparatory to pulling, was observed to stimulate the partner to pull by what has been described as *solicitation*. By this solicitation the animal was able to bring her partner to the cage grille, where she took up her rope and began pulling, or to stimulate the other ape to pull when already at the grille. Detailed accounts of the development of this behavior, together with many quotations from the original records, are given in the summaries (Bula-Kambi, pp. 29–37, and Bula-Bimba, pp. 37–42). It occurred suddenly and was clear-cut from the start. It increased in intensity, duration, and frequency with practice. We shall give a brief account of the way it was first exhibited by Bula and by Bimba.

Bula had been working with various partners for about ninety experimental sessions when it was observed that she was watching her partner with considerable regularity and intentness, and was using almost every opportunity to join her in pulling. After she had pulled successfully with Kambi for a number of trials, the first indication of solicitational behavior appeared. The box had been baited and Bula was at the grille, with Kambi in the rear of the cage. Bula pulled hard but ineffectively on her rope, then dropped it and went directly to Kambi, touched her quickly on the shoulder, and returned to the grille. Then she again took her rope, braced herself ready to pull, and turned back to look at Kambi as if expecting her to come up and help. Kambi did not come and Bula dropped her rope and played about the cage. On subsequent sessions acts of this sort were repeated by Bula, not necessarily immediately after pulling, although most often after Bula and Kambi had been working together successfully and Kambi had lost interest in the task. It is difficult to say at just what point the first genuine response to solicitation occurred. However, a positive reponse was given immediately to the third and to the fifth solicitations, and by the tenth session after the first indication of solicitation a positive response occurred quite regularly to a then well developed pattern of soliciting behavior. When the box was baited and Kambi was not at the grille to take her rope for pulling, Bula would turn and go back to her, reach out her hands with palms down, fingers flexed, and arms moving up and down. Often she was in a crouching position and bounced up and down on flexed legs. Occasionally whimpering, hooting, or other vocalizations characteristic of the excited chimpanzee accompanied these soliciting movements. Bula might touch the solicited animal or even grasp her shoulder or elbow to turn her about or pull her forward, or she might remain at a little distance from her partner and motion excitedly in a beckoning manner. The solicitation was generally continued until the partner responded by coming forward and taking a rope ready to pull. Should the partner turn away after a single heave, Bula would often begin immediately manual gestures to recall her to work.

After Bula had learned to solicit Kambi she was paired with Bimba whom she began to solicit effectively. Bimba responded to solicitation much more readily than did Kambi. After twenty-six sessions of work by this pair, Bimba gave the first indication of soliciting Bula. She had been watching Bula intently during the last few preceding sessions, and responding almost every time her partner pulled. When Bula's interest in the pulling began to lag, and she no longer solicited Bimba to work, Bimba began to solicit Bula. Bimba's first gesture appeared when Bula started to pick up her rope and then turned away without pulling. Bimba, who was sitting beside her at the grille, holding her own rope, immediately reached out, touched Bula on the neck, and turned her half way around toward the grille. She patted Bula on the neck, and Bula responded by taking her rope and pulling. Bimba continued to solicit in the following sessions, and shortly a characteristic behavior pattern developed. This pattern of of solicitation was strikingly different from Bula's in some respects, although just as effective. She always reached out for the neck of her partner, and often kept her hand there throughout the pulling in of the box. Bimba was very quiet and showed almost no excitement as she solicited. She persisted longer, on the average, in her soliciting than did Bula. Bimba used direct contact more than did Bula. She usually kept her hand resting on the neck or shoulder of her partner, while Bula character-

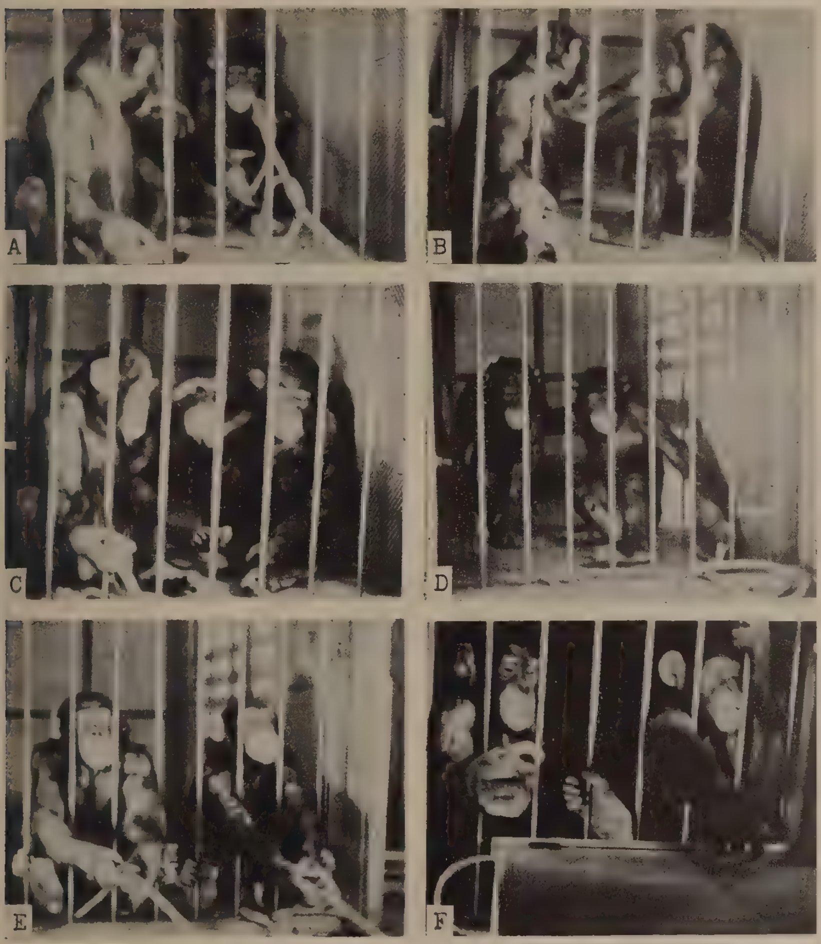

PLATE I

A, Bula beckons Bimba forward to the grille. *B*, Bula touches Kambi near the mouth, and turns Kambi's head toward the grille. *C*, Bula's hand is on top of Kambi's pushing it down toward a rope. *D*, Bimba urges Bula forward with her hand on Bula's neck. *E*, Bula and Bimba pull together. *F*, Bula and Kambi have drawn the box within reach and are taking the food. (Enlargements from 16 mm. cinema film.)

istically used a beckoning gesture, often at some distance from the other animal.

Kambi, during the last few sessions of experimentation with Alpha, made a few gestures toward her which suggested solicitation. These were not clear-cut, and with them Kambi was not able to induce Alpha to do any pulling. The behavior appeared suddenly after a number of sessions during which Kambi had been watching Alpha fairly often. Incidentally Alpha often watched Kambi at the same time.

Certain general facts about the occurrence of solicitation may be stated at this point. Both Bimba and Bula, after they had learned to solicit one of their partners, solicited, at least a few times, all other animals with whom they were paired. Thus Bula, who first solicited Kambi, began to solicit Bimba soon after they were paired. She did the same with Alpha, although she was never successful in getting Alpha to work by this method. Similarly, Bimba began to solicit when she was working with Bula. A change of partners revealed that Bimba would also solicit Kambi and Alpha, though with these two the soliciting was relatively short and entirely ineffective. It is clear, therefore, that it was not merely certain partners who were solicited, although it was, of course, done more effectively with some than with others. It appears that the use of solicitation constituted a generalized method of problem solution, applicable with any partner.

It is relevant at this point to ask to what extent solicitational behavior replaced direct pulling when the conditions for successful pulling did not obtain, that is, when the partner was not at the grille ready to pull or already pulling. How often did an animal who had learned to solicit use solicitation to get her partner to pull, rather than try to pull the box alone? Examination of the records for Bula and Kambi, between session 12, when the first clear-cut solicitation pattern and response to it occurred, and session 59, the last session on Problem I, reveals the following facts.

Bula pulled alone on the ropes, sometimes placing both ropes together before pulling, forty-one times when Kambi was not present at the grille ready to pull. She solicited Kambi forty-one different times. Four of the forty-one individual pulls were made directly after solicitation had failed to elicit immediate response from Kambi, and four of them represented the last heave of a trial, made by Bula after both animals had worked together to bring the box almost within reach. In Bula's behavior, therefore, we see that solicitation did not completely replace individual pulling, but, in point of frequency of occurrence, was somewhere nearly on a par with it.

A similar analysis was made of the records for Bula and Bimba, taken during sessions 29–36, after Bimba had learned to solicit. It was discovered that at no time did Bimba attempt to pull the box alone, while she solicited persistently on twenty-two occasions, sometimes on every heave of a trial until the box was pulled within reach. With Bimba the solicitation response seemed completely to have replaced pulling alone when no partner was at the grille ready to pull.

There is reason to believe that solicitation was initiated by the solicitor in response to the situation as a whole, rather than as an "afterthought," or as a secondary mode of response occurring when the subject was unable to move the box alone. Three solicitations by Bula of Kambi followed individual pulling by Bula within one minute; two such solicitations appeared when Bula worked with Bimba; none occurred under these circumstances when Bimba was soliciting Bula.

The intensity, duration, and frequency of solicitation undoubtedly depended on several factors, the nature and rôle of which may be revealed by further experimentation. At present we can only suggest that the following were some of those in operation. (1) *Degree of responsiveness of the partner.* Attention has already been drawn to this factor in discussing the cessation of solicitation by certain animals of those partners who consistently refused to pull when so stimulated. (2) *Degree of friendship between partners.* For example, the solicitation between Bimba and Bula was the most intense, persistent, and effective of any pair. These two apes were the best friends among our subjects. On the other hand, Alpha, who seemed to be on relatively unfriendly terms with Bimba in the living cage, was very rarely solicited by Bimba. (3) *Degree of motivation toward food of the solicitor and of the partner.* Often animals who solicited regularly for weeks failed either to solicit or to pull for a few days. (Note the disappearance of both responses simultaneously.) Probably fluctuations in the motivational value of food was an important factor in these sudden and profound changes in behavior. Also, differences in the incentive value of food for two partners might give immediate occasion for the highly motivated animal to solicit help from his less eager companion. (4) *Social dominance relationships.* The two least dominant animals, Bimba and Bula, as rated by staff members (pp. 14–15), were the two who showed the only clear-cut solicitation. They were never able to solicit the most dominant chimpanzee, Alpha, with any success. The greater success which Bimba had in soliciting Bula than Bula had in soliciting Bimba may have been related to Bimba's superior position in the dominance hierarchy. (5) *Intelligence.* Bimba and Bula have been rated by staff members somewhat higher in intelligence than Kambi and Alpha. The suggestion is made that the ability to solicit is positively related to intelligence.

The fact should be emphasized that solicitational behavior did not always take place, even when the partner had been successfully solicited many times before. The method of problem solution afforded by solicitation was by no means exploited to its full possibilities. Often a seemingly excellent opportunity to solicit was completely ignored by an animal known to be a good solicitor.

In this section we have traced the development of solution of a problem in coöperation. Sudden solution, when the problem was originally presented, did not appear. Rather, the animals went through a series of steps, the first of which was mastered with tuition from the experimenter, and each of which represented a partial solution, before a total and generally applicable solution was achieved. Only *after* all the steps had been taken did the subjects seem to "see into" the fundamental relationships involved in the problem.

VII. FURTHER CONSIDERATION OF SOLICITATION

A. Three characteristics of solicitation

In the preceding section solicitation was considered primarily from the point of view of the soliciting animal. As an adaptive response it fitted into a new chain of behavior, and, for the soliciting animal, effected a satisfactory problem solution whenever food was presented and the partner did not pull. The response of the partner to solicitation and the general social nature of the new adaptive response have not been considered in detail. That solicitation was of definite social consequence was theoretically secondary in considering the elements of individual problem solution. However, in terms of the practical situation, and with regard to the theory of social behavior, its significance is obviously great.

By way of summary, it may be well to note certain common characteristics of solicitational behavior as it was displayed by both Bimba and Bula. (1) The activity was directed at the other animal herself and not at any material object present. (2) To the observer, the soliciting animal seemed to be trying to get the partner to do something. (3) The solicitation, if successful, resulted in bringing the other animal in close spatial relationship with the rope to be pulled.

While the solicitation behavior patterns of Bimba and Bula differed widely in some respects, the activity of both fulfilled the above specifications. It appears that the differences in their behavior were either irrelevant to these fundamental similarities, or that they constituted diverse, although equally effective, ways of soliciting successfully.

B. Solicitation and amount of pulling

In many places above attention has been directed to the measurement of pulling by each subject. (See especially pp. 44–49.) A force of 160 to 180 pounds had to be applied in the horizontal direction in order just to move the box. After solicitation appeared, the two solicitors, Bimba and Bula, were observed to be pulling characteristically different shares of the total load with any of the partners with whom they were paired. Bimba seldom put more than 50 to 70 pounds of tension on the rope, while Bula consistently delivered about 150 pounds. Whenever Bimba was able to solicit help from Bula, the only partner with whom she worked at stage 3, Bula did the greater share of the work. On the other hand, Bula elicited only a very little pulling from Bimba or from Kambi, so that, although she solicited vigorously, she still did about three times as much work as her partner. These relationships in amount of pulling were most characteristic of the pulling after solicitation had appeared, although they were beginning to obtain before that period.

Various interpretations may be given to these facts. It may be that during the course of training each animal adapted itself to a certain average amount of pulling which thereafter was not influenced when that subject either solicited or was the object of solicitation. Perhaps the solicitors were never aware of the ratio of their own pulling to the total amount necessary, and hence had no way of evaluating the contribution of the solicited partner. Finally, the solicitors may never have solicited for any special amount of pulling, but simply for pulling activity itself. This was strongly suggested by the behavior of Bula, who would spend a great deal of time in soliciting Kambi to work, although Kambi repeatedly pulled less than 30 pounds and allowed slack to accumulate in her rope.

C. *Unrewarded responses to solicitation*

On five occasions Bula's solicitations were effective in starting Kambi to pull, even though food had already fallen off Kambi's side of the box before the animals had taken up their ropes. (Original records are presented on pp. 30, 31 and 33.) Of the five cases, it was clearly indicated for three of them that Kambi was working for the single piece of food, which was on Bula's side of the box, since she reached for it as the box came near, and after Bula had taken it. On the last two trials, however, Kambi only watched Bula take the food, and made no attempt to get it herself.[9]

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[9] Other cases of positive response to solicitation when no food reward seemed to be expected have been observed since the completion of this experiment.

Some motivating condition other than the desire for food is suggested by these latter observations. Kambi may have responded from a desire to rid herself of the noxious aspects of Bula's solicitation of her, or in a desire to help the other animal (altruism). It is interesting to note that, according to the ratings for dominance given on p. 15, Kambi was more dominant than Bula. Here an apparently less dominant animal obtained some unrewarded pulling (at least not food-rewarded), from a more dominant one. Such cutting across dominance relationships was also observed in the food-sharing experiment of Nissen and Crawford (1936). As in the food-sharing study, there was in this experiment no means of evaluating the possible importance of the types of motivation suggested. Hence we will have to be content with calling attention to their possible effectiveness. The need for a systematic investigation of the non-food-rewarded response to solicitation is obvious.

D. *Vocalization during solicitation*

Vocalization accompanied about one-fourth of Bula's solicitation, while it occurred only once when Bimba solicited. Bula's cries could be characterized as whimpering or whining and barking or hooting, while those of Bimba belonged only to the latter category. (For an account of chimpanzee vocalizations of these types see Yerkes and Learned, 1925, pp. 122–136; Yerkes and Yerkes, 1929, pp. 301–309; Nissen, 1931, p. 90.) The former type consists in a series of short, high pitched cries, often increasing in intensity until a scream is reached. Barking or hooting, as the name suggests, occurs with short exhalations of air, making staccato, low pitched sounds. These sounds may increase in frequency and intensity with mounting excitement of the chimpanzee, until the animal begins to yell.

It was observed that there was nothing peculiar or specialized about the vocalizations in this experiment. Bula seemed to make sounds whenever she was excited, just as she would do in the living cage, and the intensity of her vocalizations seemed directly proportional to her excitement. Occasionally she had a temper tantrum when her solicitations proved ineffective. Bimba's hooting also appeared on an occasion of some excitement, a session during which she was unusually highly motivated. The vocalizations in all cases seemed to be one of the natural accompaniments of excitement. When the solicitational behavior was well developed in both animals, and the manual behavior patterns were clear-cut, there appeared to be no differentiation in the vocalization.

E. Solicitation as communication

Many patterns of behavior similar to solicitation are used by chimpanzees in various types of social interaction. Begging for food, beckoning another animal, initiating tandem-marching, and appealing for comfort are typical activities in which such social responses occur. Examples from the literature and from laboratory notes may serve as illustrations.

Begging for food has been observed in a controlled situation, in which two chimpanzees were placed in adjacent cages and a limited supply of food was given to one (Nissen and Crawford, 1936). It was found that one animal could obtain food from the other by begging for it. Some of the subjects used in the present experiment had served previously in this food-sharing study. The patterns of begging-for-food and solicitational behavior shown by Bimba and Bula were strikingly similar, as may be seen from the following description:

> *Begging* on the part of the C-animal was characterized by extension of one arm (less frequently both arms) into the W-cage toward the W animal; occasionally the orientation of this activity shifted from the partner to the desired, inaccessible object. Typically the hand was turned palm up and the wrist, fingers, or both flexed rapidly in a beckoning gesture. Frequently the animal whimpered or whined, softly at first and then more loudly until a high pitched scream was reached. Stomping of the feet, rapid flexion and extension of the legs at the knee, and violent shaking of the grille often occurred. We found the pattern of behavior to differ considerably among our several subjects; in Bula all elements mentioned were usually present; in Bimba the vocalization was ordinarily absent, and Bimba seldom showed the finger and wrist flexing. (p. 393).

Köhler (1925, p. 313) also has given a description of the begging-for-food behavior, to which the above is similar.

Tandem-marching is often initiated by one chimpanzee with a characteristic gesture. The initiator may move near another animal, reach out toward his head or shoulders with bent wrist, and back up next to him, ready to move off in lockstep with the desired partner. This gesture has some of the characteristics of solicitation: orientation toward another animal, outstretched arm, bent wrist, and flexed elbow.

Calling other animals from a distance may also be done with a gesture involving the outstretched arm, bent wrist and elbow, and moving hand. Köhler has stated, in describing this gesture, that "the summoning of another animal from a distance is often accompanied by beckoning very human in character" (1925, p. 320).

It appears from the above examples that the solicitation observed in this experiment was not different in its essential patterning from social responses already well known in chimpanzees. It is difficult to show in what ways, if any, the pattern was specifically adapted to the pulling-in situation, beyond its obvious orientation in space. We may venture the opinion that such gestures, wherever they may occur, have essentially the same meaning for the chimpanzee to whom they are directed. Support for this opinion may be found in the observation that the solicitation in this experiment was usually responded to with activity, although not always with the same behavior. The response might be negative or positive; it might consist in withdrawal or approach, of offers to march in tandem with the solicitor, or of attempts to groom him, and, in rare instances, the solicitor might be presented with some bit of material in the possession of the solicitee. In short, it appeared that the solicitee was stimulated to *some* activity, oriented with respect to the solicitor, but not immediately with the rope-pulling. Often only after persistent solicitation did the partner take his rope and begin to pull.

It is suggested that these gestures and associated behavior make up a unit of chimpanzee communication, if we choose to call it such, which always has the same meaning. For such a unit of communication students of linguistics have used the term *recurrent same.* The meaning of the gestures seems to be generalized; it might be phrased, if it could be spoken by the solicitor, as "do something for me." It should be noted that the gestures themselves do not carry with them a directive meaning, beyond their spatial orientation peculiar to the situation in which they occur. The direction of the stimulated activity seems to be defined by the past experience of the responding animal with various features of the environmental situation.

Many writers have maintained that communication among animals is limited to a *signalizing* function (de Laguna, 1927; Esper, 1935, et al.). They hold that animal cries or gestures serve only to set off some already learned response in another animal. Instances are usually given of cries and gestures often associated with expressions of emotion, which may serve to elicit responses of flight, aggression, sex activity, and the like in other animals. The directions which such responses take are defined by the past training of the animals, and by the limitations of the situation. They believe that the communication does not serve any *predicative function* (see de Laguna), it does not describe objects, and it does not supply any information to another animal which is not already available from the physical aspects of the environment.

This general point of view seems to interpret adequately the solicitational behavior. Certainly there seemed to be no predicative function involved. The solicited animal did not seem to know what to do, and only after trying a number of responses under continued solicitation, was the pulling-in behavior given, after which solicitation ceased. It might be expected that with continued training the solicitation would be followed more quickly by pulling. While in general this was true, competing stimuli seemed to interfere, so that for various reasons the pulling response was occasionally delayed for a long time, while the solicitee busied himself with some extraneous activity.

There are many questions about the appearance of the generalized signaling pattern in this problem-solving experiment which must remain unanswered at present. The first concerns the possible stimulus which may have elicited the behavior in the solicitor. Perhaps the solicitation of another animal's help can be regarded as the selection of a tool or instrument, or as the use of some other inanimate roundabout pathway to a desired goal. This point of view would imply familiarity on the part of the solicitor with the use of other animals as means or instruments. It has already been indicated that such social sophistication was probably possessed by our subjects. The essential problem then becomes the general one of the selection and reinforcement of one of many alternative pathways to a goal object. To set the problem in this fashion offers the advantage of placing it entirely within the scope of the methodology of individual psychology.

Another important question concerns the response of the partner to solicitation. How have solicitation, begging, and other gestures given by one chimpanzee become adequate stimuli for another's activity? What drive or drives motivate such responses? Answers to these questions must await genetic and phyletic studies. At present we can only suggest that the gestures may be unpleasant stimuli which chimpanzees seek to avoid or to stop by responding appropriately; or these gestures may have come to serve as adequate stimuli for elicitation of behavior motivated by some purely social drive.

VIII. PROBLEMS II AND III: TWO-CORD AND DOUBLE-HANDLE PROBLEMS

Problems II and III differed from Problem I in complexity of apparatus and extent of individual training necessary before the subjects could be paired. The sequence of employment of these two problems is indicated in tables 1a and 1b (pp. 8–10). The possible detection of any transfer of training from one of the three problems to another, as well as tracing steps in the original solution of the two new problems, constituted the chief interests in supplementing our data with this material. In this section a brief account of apparatus, individual training procedures, and experimentation with pairs of chimpanzees will be given. The discussion will relate these findings to the results of Problem I.

A. Apparatus and procedure

Figures 3 and 4 illustrate the apparatuses used in the two new situations. An increase in the distance between the parts to be operated simultaneously was the only change made in either apparatus, after individual training in adapting it for use with two subjects. In Problem II the single subject was trained to pull down simultaneously on both *cords*, which pulled back the spring catches (*door release pins*, figure 3), and allowed the door of the *food box* to fall open. For the individual training the food box was placed before one of the front grilles, in the large restraining cage (figure 1, p. 16), and the cords hung down along the sides of that grille. When two animals were used, the food box was placed before the center of the cage and the two cords were suspended from pulleys near opposite corners of the front of the cage, one accessible through each grille. The grille in the rear of the partition separating the two sides of the reaction cage was

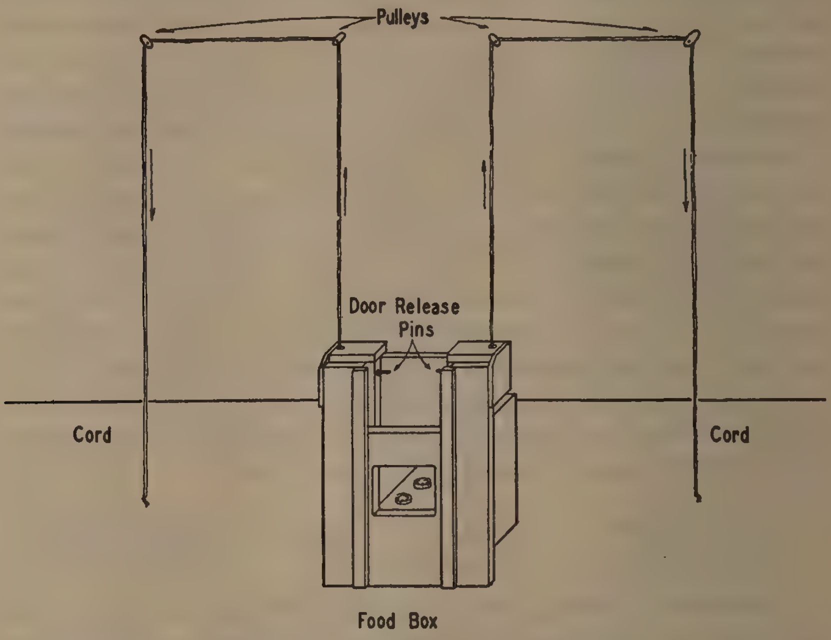

FIG. 3. APPARATUS FOR PROBLEM II

left in place or removed during various parts of the experiment, as will be indicated below.

As in Problem II, performance of the two identical acts simultaneously was necessary to obtain food in Problem III. The apparatus is represented in figure 4. Subjects were trained individually to push in simultaneously both *handles* of the apparatus when placed before the restraining cage. The handles were attached to a *cross bar* on top and at the back of the frame, which moved back when the handles were pushed simultaneously. Since this cross bar passed through a slot in the *lever*, the upper end of the lever moved back with the cross bar, while its lower extremity swung forward and brought the *food tray* within reach of the subject. The slot in the lever also served as a pivot about which the cross bar might oscillate, so that a push on only one of the handles moved the cross bar about this pivot and pushed the other handle out. While the device was best operated by careful simultaneous pushing-in of both handles, the food tray could be moved, part way at least, by pushing in one handle, holding it, and then pushing the other. For individual training a short cross bar was used, so that both handles were within easy reach of the subject. A longer cross bar, which separated the

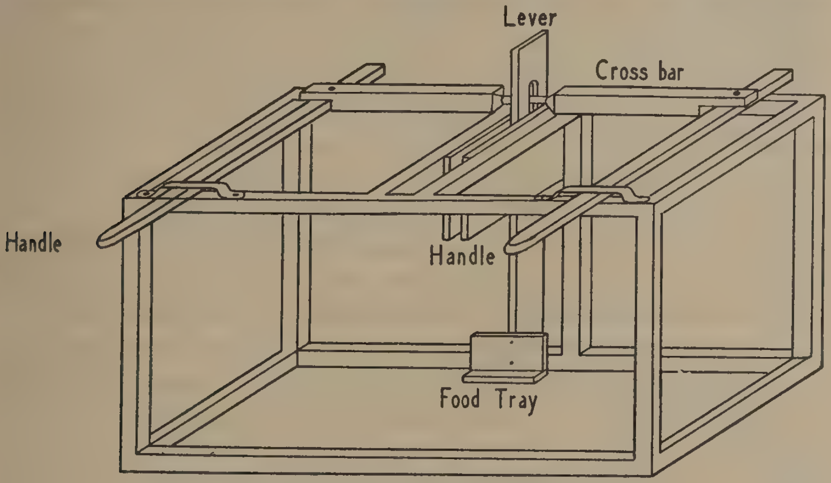

FIG. 4. APPARATUS FOR PROBLEM III

handles beyond the reach of a single subject, was used with pairs of chimpanzees.

Ten trials per session constituted the usual routine for either individual or group experimentation on both Problem II and III. Each animal was placed in one side of the large experiment cage; each subject worked an equal number of times on each side.

It is apparent upon examination of these two problem situations that individual training for them was somewhat different in nature and significance from that for the box-pulling. In the first place, in Problem II, and especially in Problem III, the animal had to become acquainted with a relatively complicated apparatus, the operation of which required movements not in the direction of the goal. By contrast, pulling in a light box with a rope appeared to present the simplest sort of a task for the chimpanzee. Secondly, during the individual training for these two problems the animals had to learn to employ the same principle that was later to be used in the coöperative solution, that is, the simultaneous operation of two mechanisms. The use of two ropes was never suggested during the preliminary training of individuals for box-pulling. The records of individual training take on considerable significance when it is realized that the individual subject could learn the fundamental requirement of the coöperative problem. The initial performances of pairs of animals on these problems are particularly interesting for what indications the records give of the immediate application of the principle of simultaneous activity already learned.

B. Results

The results will be presented in the form of summaries. In the description of the individual training on each problem all subjects will be treated together. For the experimentation with pairs of chimpanzees, a summary will be given for each pair on each problem. Throughout the summaries for Problem II the letters LC and RC will stand for the cord on the left and the right of the subject, respectively. In the accounts of Problem III, LH and RH will refer to the left and right handles.

1. Individual training

Problem II. Ross, Bula, Kambi, Alpha, Bimba, and Frank[10] were given individual training on Problem II. Tuition by the experimenter was used consistently throughout the training, whenever it seemed possible to speed up the learning by that method. Rather than presenting a summary of each animal's training, certain steps in the procedure will be indicated, emphasizing especially the rôle of the experimenter as tutor, and certain apparently natural, untutored responses to the situations will be listed.

At the beginning of training the two cords were tied together so that the subject could open the box by pulling either cord or the knot fastening them together. After this was mastered the cords were separated, at first only by a few inches, and then by successively greater distances, until at last they were at positions which would just allow the animal to reach both at the same time. Beside giving the subject an initial cue in tying the cords together, the experimenter aided the animal by placing its hands about both the cords, or, when a subject had taken only one cord, by presenting him with the second.

Certain characteristic responses of the individual chimpanzees are listed below.

1. Alternation between the two cords, pulling one and then the other many times, was the most frequent response, given by all subjects when the cords were presented separately for the first time, and often again each time the distance between them was increased.

2. One animal in particular, Bula, and the others to a lesser extent, persistently took both of the separated cords into one hand, often grasping one with each hand and then transferring one cord to the other hand, and pulled on both with a single movement. This may have been the immediate result of the training in which the experimenter sometimes handed both cords together to the subject. However, the response described next (3) occurred directly after the same type of training.

3. Kambi, and occasionally other animals, persisted in taking only one of the two cords at a time, when they were offered together by the experimenter. She would pick each cord separately from his grasp, and holding one in each hand, would pull down on both together.

4. A type of solution which appeared soon after the experimenter

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[10] Frank, a five year old male, was used only for individual training in this problem.

ceased helping the subjects, and which persisted for a long time, consisted in pulling down and holding one cord in one hand, and then taking the other and pulling it, whereupon the door would fall open.

5. Training was usually continued until simultaneous, bimanual pulling of both cords appeared. This usually occurred after response 4, above, had been given frequently, and after the time between successive pulls had been progressively shortened. This was considered the "correct" solution, and after it had appeared on a number of successive trials, the individual subject was considered ready for the problem in coöperation.

Problem III. The individual training for Problem III, given to four subjects, Bula, Ross, Kambi, and Alpha, was similar in many respects to that for Problem II. The manipulations required were somewhat different, although the essential principle of operation was the same. The types of response listed above, in so far as they were possible with the apparatus, appeared also in Problem III. Some of the subjects moved the handles freely during their preliminary exploration of the apparatus, while others had to be manually aided by the experimenter. An effective pattern of response similar to the fourth type above appeared early: the subject grasped one handle, pushed it in, and held it there while he pushed in the other. Smooth simultaneous pushing developed gradually from this pattern. One animal, however, who had used this response exclusively for a number of sessions, at the beginning of a new session, suddenly began to push the two handles simultaneously and smoothly, and continued to do so thereafter.

2. Experimentation with pairs

(a) *Problem II. Kambi and Alpha.* (Two sessions immediately after individual training, April 24, 1935.) No suggestion of appreciation by the animals of the necessity for coördinate pulling of the two cords appeared on the first few trials. The first solution came rather accidentally after six minutes. Kambi and Alpha marched about the cage for two minutes (no partition in place to divide cage), until the experimenter touched both cords simultaneously, whereupon both animals came forward and touched each cord in succession. Later they did the same thing in apparent response to his touching the cords in succession. The box finally opened when Kambi was pulling on LC and Alpha happened to pull up on RC near its attachment to the box (see figure 3), rather than pulling down on its end near the grille. On subsequent trials Kambi alternated back and forth between the cords many times, until Alpha came up and pulled the cord opposite to the one Kambi happened to have at the moment.

During the second session a pattern of behavior appeared in which Kambi typically went to LC as soon as the sliding door was raised, grasped a cord, and held it. Alpha would come up then, or later, and pull RC. This behavior seemed to develop during a time when the experimenter pointed to RC while Kambi was holding LC. The third session was given six weeks later after this pair had been working on Problem III. After Kambi had alternated between the cords a few times, Alpha came forward and pulled RC, while Kambi was holding LC. Thereafter this pattern of behavior was typical. Little, if any, watching occurred between Kambi and Alpha on this problem, so that no clear indication was given to the observer that either ape was responding to, or even aware of, her partner's activity.

Bimba and Bula. (Sessions 37 to 51, January 14 to 31, 1936.) Individual training was given these subjects during April, 1935. Retests made just previous to their work together, ten months later, indicated that each was able to operate the apparatus arranged for a single animal. Before the experimentation with Problem I was discontinued both these animals had learned to solicit the other for pulling. We were, therefore, interested to see whether any solicitation would appear in Problem II.

On the first trial, when the vertical door was raised, Bimba went immediately to LC which was her preferred side for pulling in Problem I. Bula seemed to be afraid of the metal rings placed on the ends of the cords to keep them taut, and she came up to pull RC only after pointing and coaxing by the experimenter. On the following sessions Bula went immediately to RC, pulled it down, and held it while she looked all about the cage and room, although seldom at her partner. Bimba usually came up to take LC a few seconds thereafter. The behavior rapidly became stereotyped, and continued without significant modification until the next change in procedure. During this time Bimba gave occasional indications of watching Bula, and sometimes seemed to pull LC in immediate response to Bula's pull on RC. Usually, however, each animal went to a part of the task without apparent regard for the other. Since Bula would take both pieces of food if two were placed in the box initially, only one piece was placed there, which Bula took, and afterward another was deposited for which Bimba reached while Bula was busy eating the first. Since both animals usually pulled their respective cords soon after the door was raised, there was no occasion for solicitation, or, indeed, very much watching. After the first three sessions, the time taken to open the box varied between 2 and 14 seconds.

On session 42, a piece of rope with a small metal hasp attached to one end was fastened to part of the rear of Bula's cage. It was intended that this should serve as a play object for Bula, who might manipulate it instead of coming forward, and thus delay the coöperative act. Bula seemed afraid and did not play with the hasp, so it was ineffective in causing delay. When introduced on Bimba's side on the next session, it attracted her attention so that when the door was raised and Bula went immediately to RC, Bimba continued to finger the strange object. After waiting for some time at the grille for Bimba, and watching her as she played, Bula went to the rear of her cage and, reaching through the back grille, began to solicit Bimba in a manner characteristic of Problem I. The original record follows:

> Bula came forward when the door was raised and pulled on RC for a few seconds. She ran back and reached through the back grille, touching Bimba with palm up and wrist flexing. Bimba immediately moved to the front grille, and Bula started forward also, but returned to the rear, reached through the back grille, and fingered the plaything. Bula went forward, as Bimba came back, and pulled on RC again, and looked back toward Bimba, and returned to the rear once more. She reached through the back grille and solicited Bimba a second time in the characteristic manner. Bimba did not respond this time, and Bula went back to the forward grille. A third attempt at solicitation was made by Bula at 1′ 45″, to which Bimba did not respond. Bula went forward and remained there, pulling occasionally on RC, sometimes looking toward LC. At 2′ 30″ Bimba left her toy and came somersaulting to the forward grille, took LC, and pulled. Bula did not see Bimba come forward, though when she did notice her at the grille holding LC, she immediately pulled RC.

This particular distraction object was not effective when introduced subsequently.

On session 49 a new play object was introduced on Bimba's side. The back grille had been removed from the partition, allowing free movement from one side of the cage to the other. Solicitational behavior was observed twice when Bula tried to get Bimba to pull LC. She solicited for more than five seconds each time, but was unsuccessful in coaxing Bimba away from the plaything up to the grille to pull her cord. After Bula had pulled both cords many times and one remained stuck when it was pulled down, the box finally opened. On the next session a new plaything was given to Bimba, with which she busied herself for twenty minutes. Bula solicited her briefly once, though not successfully. Bula alternated back and forth between the cords, never opening the box. On the last session no play objects were used, and ten trials were rapidly completed in the stereotyped manner.

(b) *Problem III. Kambi and Alpha.* (Twenty-two sessions, May 20 to June 24, 1935.) Individual training of Kambi and Alpha on the double-handle apparatus immediately preceded their use together in the situation. Aside from their use in Problem II for two sessions, they had not worked in any coöperative problem before. Hence, it was with this pair that we could observe the development of joint activity almost from the start. This summary therefore should be compared with that for Ross and Bula on Problem I. For the early sessions the back grille in the partition was not in place, and the subjects moved freely from one side of the restraint cage to the other.

The activity on the first trial was similar to that at the beginning of testing on Problem II. Kambi ran back and forth between the handles, as did Alpha, though to a lesser extent. The first solution occurred when both animals were standing near RH; Alpha took hold of RH and started to push it and Kambi ran to LH and pushed it in. Subsequent sessions followed much the same course, with less running from one handle to the other. On sessions 2 and 3 both animals came to the grille at about the same moment, though Kambi often had time to shift rapidly from LH to RH and back to LH before Alpha took RH. When Alpha took RH she usually pushed it in and held it there until Kambi had pushed in LH. During these trials neither animal gave any indication of watching the other, making it impossible for the experimenter to tell whether or not they were responding to each other's activity. A strong preference for a particular side was shown by each animal: Kambi for the left and Alpha for the right. The fact that each immediately went to one side when the door was raised does not, therefore, indicate any special awareness of the other animal's rôle. Often when an animal arrived at the front grille and grasped a handle, she would push it in and thereby push out the first's handle. They might keep up this see-saw motion for some time, until, because their pushes overlapped for an instant, the food tray was moved within reach.

This type of behavior was characteristic of the next five sessions. On the sixth it seemed that both animals were watching the lever as they pushed. Very few changes in side, or alternations, occurred thereafter. Gradually a pattern response developed in which Alpha usually came up and held RH or pushed it a little after Kambi had been working LH in and out. By the eleventh session this behavior was quite common, deviations from it taking place only when Alpha seemed to lack motivation and did not come forward soon after the door was raised.

The partition in the middle of the restraint cage was installed before the twelfth session, and remained in place thereafter. On this session Alpha spent the entire time sitting on the framework of the partition. Kambi made no attempt to solicit Alpha's help at RH; in fact, she never seemed to notice Alpha, except once when she climbed the partition herself. On the third trial of session 13 Kambi was observed, for the first time, watching Alpha. She was holding LH and Alpha was sitting on the partition whence she climbed down and moved over to grasp RH, while Kambi watched her intently. On the sessions thereafter Kambi seemed to go more directly to LH and, in holding it, to wait for Alpha to push in RH. Occasionally she turned to watch Alpha come forward.

The remaining sessions (14 to 22) were characterized by some watching of Alpha by Kambi, and a little of Kambi by Alpha. Watching appeared particularly when something went wrong, as when one of the animals did not come to her handle for some time, or when the animals did not push coördinately. Coördination probably was facilitated to some extent when the animals watched the lever rather than each other, since the lever moved only with coördinated pushing. No indications of solicitation appeared, though one animal might wait for some minutes for the other to push its handle.

Bula and Ross. (Nine sessions, June 19 to 24, 1935.) Bula and Ross were given individual training just before they were paired in this problem, and immediately after they had completed seventy sessions on Problem I. In Problem I Bula had learned to watch Ross and to time her rope pulling with his. Interest was centered in noting whether or not watching behavior would appear when Problem III was presented. This pair had received no training on Problem II.

On the first session each animal attacked the handles with vigorous pushing. Each went many times to both handles (the partition was open), so that they changed positions every few seconds after pushing a handle. They touched each other in passing, looked at each other casually, but they gave no indication of watching one another in order to coördinate pushing. The food tray was usually brought within reach by a series of see-saw pushes, as described above for Kambi and Alpha.

Bula watched Ross on many occasions during the second session, and she sometimes held RH, while watching him as he came up to push LH.

Since they did not push the handles all the way in a smooth, coördinated manner, but see-sawed the cross bar back and forth, it appeared that Bula might understand that Ross must be at the other handle, but that she did not understand just how the handles were to be manipulated simultaneously. By the third session a steroetyped response was beginning to form. Bula came to RH, held it, and watched Ross come up and push LH. Often when she did not watch Ross, she concentrated her attention on the lever. This type of behavior persisted through the eighth session. The actual manipulation of the handles by both animals was faulty, and many seconds were required to get the food tray within reach even after each animal had grasped a handle. Two factors contributory to poor joint manipulation were: Ross' jerky pushing on his handle, and Bula's reaching toward the food tray before it came within reach. On the ninth session a shorter cross bar was substituted. This made manipulation easier and considerably reduced the time taken on a trial to bring the food tray within reach.

Bula and Kambi. (One session, June 25, 1935; fourteen sessions, nos. 60 to 68; 70 to 74, January 22 to February 10, 1936.) Before the first session with this pair Bula had been working with Ross on Problems I and III, and Kambi had been trained with Alpha on Problem III, followed by Problem I. Each of these subjects had played the rôle of coördinator, to some extent, in their former partnerships, as evidenced by their watching and responding to their partner's activity. This was not true for Kambi in Problem I. Part of the record of the first session on this problem is presented.

> Each animal came to the front grille and held one of the handles, Kambi LH and Bula RH, and looked at the other as if waiting for her to make the first move. Then both began to push in slowly until the food tray came within reach. Bula seemed to watch Kambi more intently than Kambi watched Bula.

In this manner it was suggested that the nature of the rôle played by each animal in former pairings was a determinant of their initial behavior in the new grouping. Watching continued throughout the first session; the average time taken to bring in the food tray during 10 trials was 5.7 seconds.

The next session was not until six months later, at which time the animals were given individual retraining before being used together. In this new series of sessions a stereotyped pattern of behavior soon formed in which Bula characteristically came forward, pushed RH and turned to watch Kambi come forward and push LH, sometimes after first looking at Bula. Often, however, the action of the animals was simultaneous, and during it they usually watched the lever and occasionally each other. A quick response, sometimes involving watching by both animals, was characteristic of the later sessions.

Beginning with session 62, play objects (similar to the ones used with Bimba and Bula in Problem II) were given to one of the animals, in an effort to delay the joint activity and to place watching and solicitational behavior at a premium. While watching by the animal who did not have the play object, but was at the grille and holding her handle, apparently waiting for the other to come up, often occurred, no solicitation ever appeared. Session 69, during which Bula solicited Kambi for pulling, was used to take moving pictures of the animals working on Problem I. This is particularly interesting since Bula never solicited on the preceding and following sessions with Problem III, even though it would have been very useful. In some of the sessions, the partition was open. When a play object was placed in the cage, Kambi would busy herself with it while Bula went back and forth many times between the handles.

Bula and Bimba. (Four sessions, nos. 52 to 55, February 12 to 17, 1936.) Bimba had not received any preliminary individual training on Problem III when the group testing was begun. It was therefore interesting to see whether Bula would attempt, in any way, to indicate to Bimba what she was supposed to do. At the beginning of each of the four sessions Bula took her position holding RH and looked around toward Bimba, as she had done for Kambi. Bimba fingered LH twice on the first session, and she was apparently somewhat frightened when it suddenly moved out toward her as Bula pushed RH. Thereafter Bimba usually remained in the middle, or toward the rear, of her side of the cage, and played by herself without regarding the apparatus. Bula occasionally solicited Bimba through the back grille in the same way she had done on Problem II. During two of these solicitation periods Bula seemed to push on Bimba's back in such a way as to urge her forward. Beside this, Bula made no motions which might be interpreted as tuitional. When Bimba failed to respond to solicitation Bula desisted and began to play about on her side of the cage. Before the last session the tray had been enlarged, and an increased amount of food placed thereon. Bula seemed to be highly motivated and pushed her handle in and out violently. Bimba came forward and pulled on the frame of the apparatus, giving every indication of being eager for the reward. Bula solicited Bimba on a number of occasions, but was never observed to try to indicate to Bimba what she should do. The animals did not obtain the reward at any time in this problem situation. For various reasons it appeared that Problem III was not well adapted for a test of possible tuition of an untrained chimpanzee by a trained one, and hence its use was discontinued.

C. Discussion

Under this heading certain topics will serve to sum up the experimentation on these two auxiliary problems and at the same time to indicate points at which the data supplement those already presented for Problem 1. The two problems used required simultaneous movement of two similar parts of an apparatus by two animals. Subjects, who had been used also in Problem I, were trained individually to operate the apparatuses when adjusted so that both moving parts could be simultaneously manipulated by the two hands of the animal.

Although during the individual training each animal learned not only how to operate a single mechanism, but also how to coördinate the operation of two similar parts of an apparatus, the initial performance of Kambi and Alpha (who were the only subjects for whom Problems II and III were the first problems in coöperation), as well as that of some other pairs, was entirely uncoördinated. Each animal alternated back and forth between the cords or handles, or worked at one mechanism without apparent attention to the other chimpanzee. No transfer, from individual training, of any notion of simultaneous activity was indicated. The only appropriate element of behavior which seemed to be carried over was the act of pulling a cord or pushing a handle. No control experiment was carried out by which this general negative statement could be verified. Had a similar group of subjects been trained to obtain food by pushing a single handle or pulling a single cord, and then introduced into the coöperative situation, results might have been obtained which could be directly compared with the data already collected. Such comparison might indicate that something more was transferred from the two-cord or the double-handle individual training than was apparent.

The development of coördinated activity in these two situations, particularly as observed in Problem III, followed a course similar in general outline to that described for the original training on Problem I (see pp. 49–62). Without attempting to press the similarity far, certain stages in the training of Kambi and Alpha on Problem III, which correspond roughly to the untrained behavior and to stages 1 and 2 recognized in Problem I, are presented.

1. Alternate manipulation of the two handles for long periods until, by chance, one animal held one of them for an instant while the other pushed in the second handle.
2. Observing the lever, in the center of the apparatus, and apparently responding to its movement by continuing to push the handles.
3. Watching the other animal and pushing a handle after the other had already pushed one.

In the summaries on individual training (pp. 73–74) it was noted that the sequence of learning the bimanual coördination seemed, to some extent, similar to the one outlined for the learning of Problem I and III. Stages characterized by the adjectives alternative, successive, and simultaneous could be distinguished. An analogy between the learning of bi-manual and bi-individual coördination is suggested.

One of the original questions of this investigation concerned possible *transfer of training* from one problem in coöperation to another. Would pairs of animals trained on one or more coöperative problems solve a new problem more quickly than animals not so trained? After some experimentation it was discovered how difficult it would be to set up a series of well controlled situations to answer this question. With a small number of subjects (in comparison with the statistically reliable groups ordinarily employed in such studies), and a limited number of problems which might be used, clear-cut evidence probably would be hard to obtain. The problems should be equated, more or less, for mechanical difficulty, and should be designed to place a premium on the transfer of "coöperativeness," rather than of specific motor skills. "Coöperativeness" might include an awareness of a partner's possible function in any problem situation, and a dependence on him for sharing the labor.

The present investigation has been exploratory, and the data from it do not afford any unequivocal indication of transfer of training from one of the three problems to another. It is true that in certain problems some animals, who had been trained previously in other problems, displayed watching behavior in a new situation earlier than those subjects who began their training on that problem. For example, Bula watched Ross during the second session of their pairing on Problem III, after finishing seventy sessions on Problem I, in which the watching first appeared, while Kambi did not watch Alpha until the fourteenth session of their work together on Problem III, on which they were tested first. When Kambi and Alpha were used on Problem I, however, after Problem III, watching did not appear any earlier than it did with Ross and Bula on the same problem. In fact, the entire tuitional procedure, including the use of two boxes, help from the experimenter, etc., had to be employed with Kambi and Alpha. Concerning the appearance of solicitation on new problems after its observation on other problems, we can report that Bula occasionally solicited Bimba when the need arose in Problem II (when Bimba was manipulating one of the play objects rather than coming forward to pull a cord), but she did not solicit Kambi under similar circumstances in Problem III, even though on the same day she solicited her for pulling in Problem I. An attempt at quantitative measurement of possible transfer to Problems II and III from Problem I, employing solution times of individual trials, was found unsatisfactory because the use of play objects resulted in artificial delays of the coöperative acts.

Little transfer of training between the various problems of this experiment seems indicated, but the data are entirely inconclusive on this point. Testing this very interesting possibility must await a new study, in which more animals are available for use and problem situations are more carefully chosen. In such a study the development of "coöperativeness" in chimpanzees might be followed.

Finally, brief comment should be made on the results of the four sessions in which Bimba, in combination with Bula, received her first test on Problem III. While Bula vigorously solicited Bimba a number of times, in the same manner as she did in Problems I and II, where Bimba already knew what to do, she gave very little, if any, indication in Problem III of trying to show Bimba what to do. She reached through the grille on two occasions with the characteristic soliciting gesture, and she appeared also to be trying to push Bimba forward toward the apparatus.

This limited observation gives some substantiation to the arguments presented above in the discussion of communication. It was suggested that solicitational behavior represented a unit of meaning ("the other animal wants me to do something for her"), which served to set off or signalize an activity already learned. It appeared here that solicitation was limited to a signalizing function and was not, in any way, adequate for directing an animal to perform an act new to it. Experimentation obviously should be continued in the exploration of various situations which may afford satisfactory opportunities for elicitation and observation of tuitional behavior on the part of chimpanzees. For various reasons, Problem III was not well adapted for this purpose.

IX. SUMMARY

Young chimpanzees were first trained individually in a problem situation until they had thoroughly mastered the manipulations required. The problem apparatus was then arranged in such a way that one animal could no longer operate it alone, whereas two animals, working together, could successfully manipulate the device. Initial attempts of pairs of animals to operate the mechanism were entirely uncoördinated. This study was directed at a description of the learning of pairs of chimpanzees to coöperate. Three problem situations were employed in irregular order: I, box-pulling problem; II, two-cord problem; III, double-handle problem. Problem I was used for intensive study, while Problems II and III furnished supplementary data.

In Problem I five young chimpanzees were trained individually to pull in with a rope a box baited with food. The box was then made too heavy for a single animal to move, two ropes were attached to it, and it was presented to pairs of subjects. Initial attempts to draw in the box together were unsuccessful, and were characterized by alternate or entirely uncoördinated pulling, rather than the requisite simultaneous activity. Training procedures were begun, in the first of which two light boxes were presented instead of the single heavy one. These the subjects soon learned to pull at the same time, although independently. Replacing the two boxes by the single heavy one, training was continued as tuition by the experimeter, who first helped the subjects by pushing the box, and then gradually reduced the part he played in the situation until only the command "pull" was given. Simultaneous response by both partners to this auditory cue characterized stage 1 in learning to coöperate. Stage 2 was recognized when one member of a pair began to watch the partner and to time its own pulling with that of its companion. Stage 3 appeared when a member of a pair not only coördinated with a partner when he pulled, but also solicited him for help when he was not pulling. Stages 2 and 3 developed without tuition by the experimenter. Of the five subjects, all reached stage 1; in certain combinations four showed behavior typical of stage 2; and two solicited all partners with whom they were paired, or reached stage 3. Of the seven pairings of subjects, one never worked together successfully, two reached stage 1, two stage 2, and two performed consistently at stage 3.

Solicitation was exhibited by each of two chimpanzees in a different and characteristic manner, although with the same result. One usually reached toward her partner with outstretched arm, moving in a beckoning gesture, with fingers and wrist bent, often touching the other animal on head, shoulder, or elbow as if to turn her around. In this solicitor excitement was frequently indicated by jumping up and down with flexed legs, and by her facial expression and occasional vocalizations. The other solicitor almost invariably used a single movement: she reached for the back of her partner's neck and seemed gently to draw or turn her toward the grille and ropes. She vocalized only once and never seemed to be excited. After solicitation appeared it replaced pulling alone, when the other animal was not pulling, in about half the cases with one solicitor, and in all the cases with the other. The intensity and duration of solicitation probably depended on a number of factors such as: responsiveness of the partner, degree of friendship between partners, strength of individual motivation toward food, dominance relationship, and intelligence. The vocalizations accompanying solicitation were those characteristic of the excited chimpanzee and they seemed to have no special directive significance when they appeared with the signalizing manual gestures. The pattern of solicitation strongly resembled other signaling behavior of chimpanzees, especially that used in begging for food or other desired objects. The solicitation was occasionally effective in obtaining help from another animal even when the latter did not receive food as reward. The amount of pulling done by partners was not divided equally; one solicitor seldom pulled more than one-third the total load in any partnership, while the other did almost all the work when paired with any other animal. The patterns of pulling, as represented in the graphic records, showed characteristic differences for each subject, as she worked alone or in combination with another.

In Problem II individual animals were trained to pull, bimanually, on two cords which released the door of a food box. The cords were then separated so that they could not be reached simultaneously by a single animal, and two chimpanzees were placed in the situation. Six subjects, five of whom were used in Problem I, were given individual training, and two pairs were used in experimentation. Problem III required simultaneous pushing on the two handles of an apparatus, which resulted in bringing a food tray within reach. Four subjects, all used in Problems I and II, were trained individually, and the behavior of four pairs was studied.

The initial behavior of pairs of subjects in these problems gave no indication that the principles of coördination learned bimanually by each subject during individual training were transferred at once to the paired situation. Learning to coöperate followed a course similar in outline to that described for Problem I: simultaneous response to an *external* cue, watching the partner, and solicitation of the partner appeared in order. One pairing of subjects in Problem III included an animal previously untrained in that problem. Its partner unsuccessfully solicited its help a few times, but gave no clear indication of trying to show the untrained partner what to do. It appeared that the solicitational behavior served only as a signal to set off an already learned response, and had no directive influence.

This experiment was not designed in such a way as to reveal any possible transfer of training from one problem situation to another, and from the results it may not be inferred that such transfer did or did not take place. The most significant result of the inquiry is the discovery that young chimpanzees will employ a pattern of behavior, which has been observed in other social situations, to solicit help from another ape in a problem situation requiring coöperation.

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Factors in Rat Learning

An Analysis of The Intercorrelations Between 34 Variables

BY

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FACTORS IN RAT LEARNING: AN ANALYSIS OF THE INTERCORRELATIONS BETWEEN 34 VARIABLES

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I. INTRODUCTION

Numerous measures of variables such as learning ability, motivation, wildness, persistence, and general activity have been devised in animal work; but little attempt has been made to determine what these commonly-used tests actually do measure. This study was designed as a preliminary attempt to discover whether we can disclose by the factoring methods what they do test and what they have in common.

In using the present factor methods, one assumes that an animal's score on a test is a linear function of its score on a complex of factors (18), that is,

$$S_{ji} = a_{j1}x_{1i} + a_{j2}x_{2i} + a_{j3}x_{3i} + \cdots + a_{jq}x_{qi},$$

in which S_{ji} represents the standard score of individual i in test j, the x's represent standard scores in the q statistically independent arbitrary reference factors, and the a's represent factor loadings in the tests. With this assumption made, the problem then becomes one of determining the parameters, a_{jq}, of the equation so that they are psychologically meaningful.

Dunlap's (2) study with chicks, Thorndike's (17) and McCulloch's (11) with rats, and other intercorrelation studies with animal subjects, extensively reviewed by Thorndike (17), bear directly upon the problem of this experiment; but newer developments in factor theory make it seem advisable to postpone discussion of their results until further analysis has been made. It is doubtful, however, whether these earlier studies will reveal more than a tentative list of factors which are operative in producing the scores on the various tests. Those studies, as well as this one, will merely serve as a basis for follow-up work along lines that might be expected to yield more definite results.

The tests used in the present study were chosen in an attempt to isolate as many factors as possible with previously used animal tests. Those already in use were selected in order to avoid setting up for the present at least, new and perhaps entirely unreliable measures of hypothetical functions. The following discussion will be divided into four sections: Apparatus and Procedure, Variables and Intercorrelations, Factor Analysis, and Discussion.

II. APPARATUS AND PROCEDURE

Subjects

The subjects for the experiment consisted of eighty male rats, started in six groups of 12 each and one of eight, not all of which completed training. The animals were fourth to sixth generation offspring of a cross between rats from the Wistar stock and trapped wilds, taken at random from the colony of the psychology laboratory of The University of Chicago. However, runts and others obviously physically defective were excluded. The total was composed of about thirty litters. The ages of the animals when taken from the colony ranged between 79 and ninety days. The dispersion increased somewhat during the period of experimentation.

TABLE I

Dates at which animals started on various tests

RAT NUMBERS*	WILDNESS	ACTIVITY	STRAIGHTAWAY	FIRST RIGHT GOING	LEFT GOING	SECOND RIGHT GOING	MULTIPLE —T MAZE	RECTANGULAR MAZE	PROBLEM BOX	LIGHT DISCRIMINATION	MAIER TEST	PERSEVERANCE
1–12	12–11 '33	12–15 '33	1–6 '34	1–9 '34	1–15 '34	Varied	2–7 '34	2–28 '34	3–16 '34	4–11 '34	—	Varied
13–20	3–16 '34	3–20 '34	4–12 '34	4–17 '34	4–29 '34	Varied	5–16 '34	6–8 '34	6–26 '34	7–21 '34	—	Varied
21–32	4–9 '34	4–13 '34	5–7 '34	5–11 '34	5–26 '34	Varied	6–20 '34	7–21 '34	8–8 '34	8–24 '34	—	Varied
33–44	10–14 '34	10–18 '34	11–9 '34	11–14 '34	11–26 '34	Varied	12–22 '34	1–15 '35	2–1 '35	2–25 '35	Varied	Varied
45–56	11–4 '34	11–8 '34	11–30 '34	12–5 '34	12–17 '34	Varied	1–13 '35	2–5 '35	2–17 '35	3–12 '35	Varied	Varied
57–68	12–2 '34	12–5 '34	12–29 '34	1–1 '35	1–15 '35	Varied	2–7 '35	3–1 '35	3–17 '35	4–7 '35	Varied	Varied
69–80	12–18 '34	12–22 '34	1–15 '35	1–18 '35	1–30 '35	Varied	2–21 '35	3–14 '35	3–30 '35	4–21 '35	Varied	Varied

* Some animals died during experiment.

General procedures

The animals were started at seven different dates. They were taken from the breeding cages on the following days: December

11, 1933, and March 16, April 9, October 10, November 1, December 1, and December 18, 1934. Table I gives the approximate dates at which each group started on each test. As the experimental period lasted five months, initial and daily weight records, from the third day on, were taken to prevent any marked inanition or overfeeding.

Temperature control was by thermostat. The regulator was set at 78°, but the temperature ranged between 75 and 95° in the room during the period from December 11, 1933, until June 1, 1935. However, while the animals were in the activity cages, and most of the other time, the temperature was constant at 78°. Ceiling lights (with 100 watt bulbs) served for illumination of the room during experimentation.

The incentive on every task was bread and milk. The animals were fed during and after the completion of each day's runs. In general, after finishing the various tasks, they were given a three-day rest period with lettuce and meat in the home cages. Exceptions to this rule will be noted in the section for specific procedures. The writer cared for the animals and ran them through all the tests.

Apparatus and specific procedures

The apparatus for the experiment involved ten different set-ups. They were, in order of use, Wildness Tunnel, Activity Cage, Straightaway, Simple Maze, the 12-cul-de-sac Multiple-T Maze, Rectangular Maze, Double Platform Problem Box, Multiple-Light Discrimination Box, a modification of Maier's "reasoning" test, and Perseverance Box. The animals were trained for three habits on the Simple Maze. In general, the scores taken were total time, time in errors, errors, and trials to meet a criterion. Description of the apparatus and of specific procedures follows.

Wildness Tunnel. This apparatus is the same as that devised and validated as one measure of wildness by Stone (16). He found that time in the tunnel differentiates trapped wilds and second generation crosses between wilds and tame albinos from tame albinos. The two former groups, and especially the first, remain in the tunnel much longer than the last.

The apparatus used was merely a U-shaped piece of four-inch stovepipe, as illustrated in Figure 1. When taken from the colony, the animals were weighed and on the following day fed, four at a time, for 15 minutes in the food box of the apparatus. Inasmuch as some of the tryout animals, without preliminary feeding, remained in the tunnel for long periods, the preliminary feeding with the experimental subjects was continued for three days to lessen the time for giving the test. On the fourth day each animal was placed in the entrance (S) of the tunnel and allowed to remain there until fully entering the food box. The score recorded was the time elapsing between placement in the entrance and the exit into the food box. Animals remaining in the tunnel for 200 seconds were taken out and that score recorded for them. Time was taken with a Brenet stop watch. The experimenter stood at all times in front of the exit.

Activity cages. The activity cage consisted of a 14-inch sheet metal disc, tilted at a slight angle and mounted on the axle of a bicycle hub. The axle was attached by a shaft and eccentric to a Veeder counter, which registered complete revolutions of the wheel in either direction. Surrounding the wheel, with one-fourth inch intervening, was a vertical cylinder of galvanized sheet iron. A removable cover of wire netting prevented the animal's escape. The rat could obtain water at all times through a tube leading from an inverted glass jar placed on the cover of the cage. A set of 12 of these cages was mounted on three shelves of a rack. Figure 1 shows the design of the apparatus.

The animals were placed in the cages immediately after being given the trial in the Wildness Tunnel. They remained in these cages 14 days, being taken out and placed in their home cages for feeding at a given time each day. They were allowed to eat for one hour and a half. After being fed, each animal was placed in a cage different from the one in which it had been the preceding day. For instance, on the first day Rat 1 was placed in Cage 1, on the second day in Cage 2, on the third day in Cage 3, and so on. Bread and milk, with lettuce and meat on the seventh day, was the diet.

The animals were allowed nine days to get accustomed to the

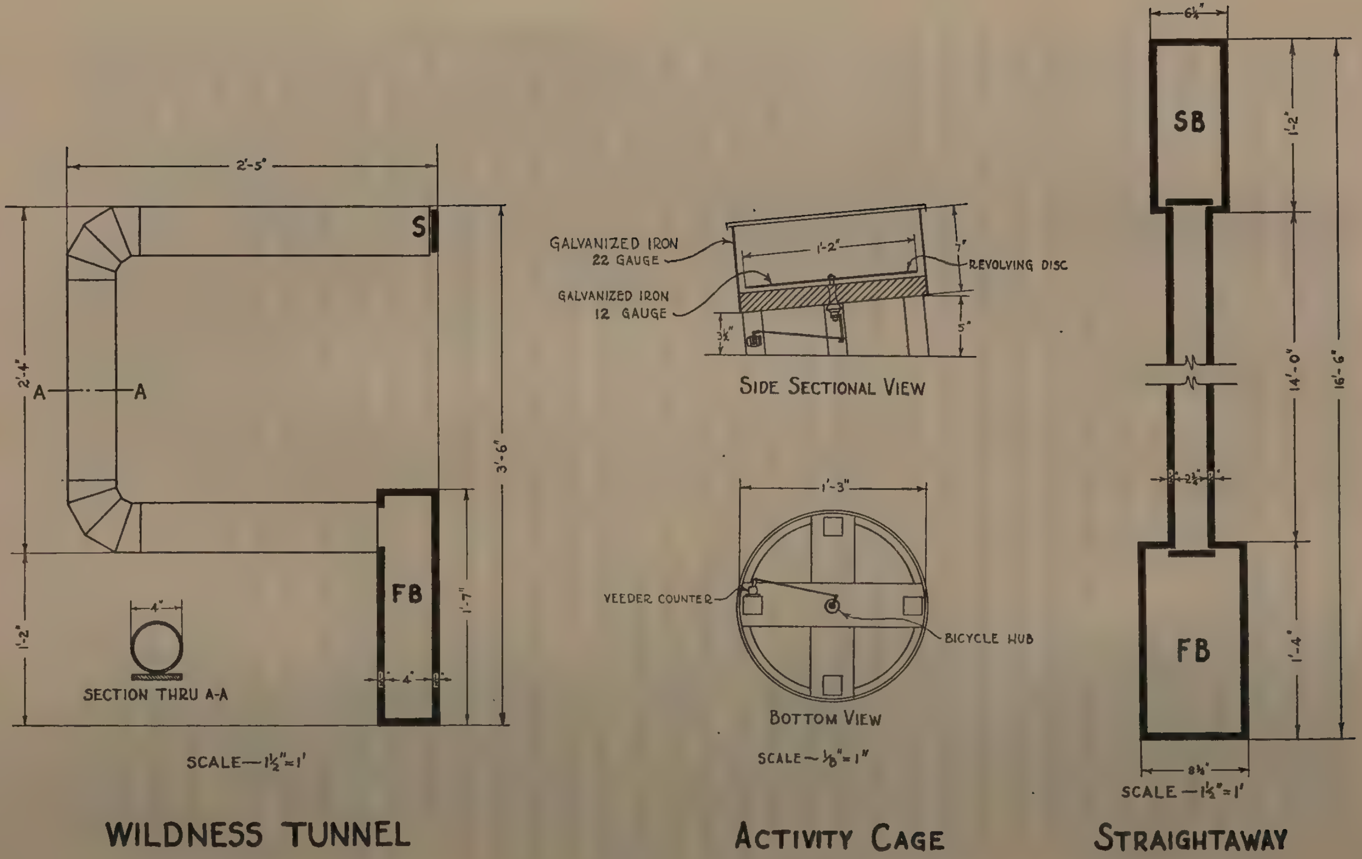


Fig. 1. Diagrams of Apparatus

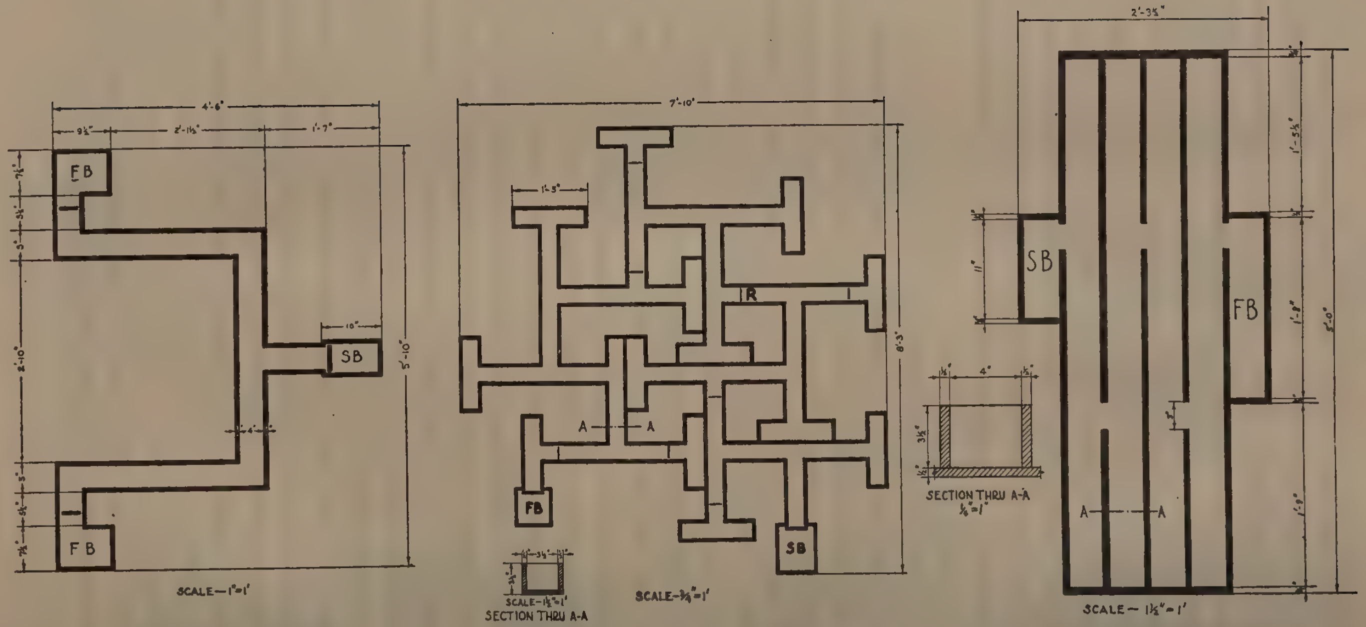
cages. This procedure is in accord with that of Shirley (13) with a similar apparatus. For the present study records were kept of the total activity, days 9 to 14. These scores, although utilized primarily as a measure of the spontaneous activity of the rat, are undoubtedly influenced by various factors, an important one of which is probably bodily endurance.

Straightaway. The Straightaway, used here to measure the rat's speed of running, was 14 feet long and of the design commonly used for preliminary training in maze experiments. The starting box, with a release door, was at one end of the path; and the food box, with a trap door, at the other. The measurements are shown in Figure 1.

In general, three days of rest were given the animals after they were taken from the activity cages. During this period, they were fed meat and lettuce besides the regular bread and milk diet. The animals were then given three days of preliminary feeding, 15 minutes a day in the food box of the Straightaway. On the fourth and fifth days, they were given five trials on the Straightaway and allowed to eat for 15 minutes thereafter. The sixth day they were fed only the bite or two of food which they received at the end of each of the five runs. This procedure was used to minimize the number of retracings on the record day. The time spent in the Straightaway trials 15–20, on the seventh day, was the measure of speed which was recorded for the intercorrelations. The score used was the number of seconds elapsing between the complete entrance into the Straightaway and complete entrance into the food box. Records were not used for the first 12 animals because of errors in timing.

It became apparent after observation of the rats' behavior even on trials 15–20, with strong motivation, that the scores would not measure pure speed of motor reaction. Retracings were still made in many instances. In others, some of the rats were apparently afraid to go through the trap door at the end. Apparently, wildness and learning elements still entered into the rats' behavior.

Simple maze. This maze is the same as that used by Leeper (10) and was included in an effort to measure the rat's ability



SIMPLE MAZE

MULTIPLE-T MAZE

RECTANGULAR MAZE

FIG. 2. DIAGRAMS OF APPARATUS

to make a simple association. Figure 2 shows the design. Upon being released from the starting box of the apparatus, the animals could go either right or left and proceed to the end without further choice. The rats were trained for three habits on this maze. On the first set-up, the right going response was correct; on the second, the left-going; and on the third they were trained to go back to the right—in all, two reversals plus the original right-going habit.

The animals began preliminary feeding in the right food box the day after they had finished with the Straightaway. They were fed 15 minutes on each of two days. On the third they were given one trial and allowed to eat for 15 minutes upon completing the run. On the fourth, they were given five trials and fed for ten minutes. Thereafter they were given ten trials a day until reaching the criterion of mastery. The criterion was ten consecutive errorless runs to the right. Animals not meeting the criterion in one hundred trials were discontinued on this problem and started on the next. Only seven minutes of supplementary feeding were given during the ten-trial-a-day training period. Four scores—total time in alleys, time in errors, number of errors, and trials to meet the criterion—were kept. Only entrances farther than the end of the elbow of the blind were recorded as errors, the time-in-error score being the number of seconds during which the rat stayed in the cul. Total time included this period plus the remaining time elapsing after the animal had left the starting box and completely entered the food box. Time records were discarded for the first 12 animals because the clock used was found to be in error.

The animals were given a three-day rest period after the poorest of the group had formed the right-going habit. They were then given five re-enforcing trials to the right. The left-going response was then made correct, five trials to the left immediately following those to the right. The animals were then given ten trials a day on the left-going habit until mastery. The criterion, as before, was ten consecutive errorless trials. Time in the maze, time in errors, errors, and trials wêre recorded, the same type of error and time measures being used as previously. The timing method was the same as before, records from the first 12 animals being discarded.

Each animal was reversed to the right immediately after meeting the criterion for the left-going habit. The animals were reversed after one hundred trials if they had not met the left-going criterion before. Scores recorded and the criterion on the second right-going habit were the same as on the left-going and first right-going habits. The antagonistic habits were set up in an attempt to obtain some measure of the animals' reversal ability. Three days of rest were allowed after this task was completed by the slowest animal.

Multiple-T maze. The 12-cul-de-sac Multiple-T Maze was the seventh test in the series. The apparatus is shown in Figure 2. It is practically the same as that used by Stone and Nyswander (15), Tolman and Nyswander (19), Leeper (9), and others. It appears to be one of the most reliable tests of learning ability in the rat and was included as a test of that ability. Doors to prevent retracing were placed at the four points indicated in the drawing. False doors to obviate, somewhat, systematic visual cuse were included.

The procedure was essentially the same as in the preceding tests, with three 15 minute preliminary feeding periods and one trial on the fourth day. The animals were given only five trials a day thereafter, however, and the criterion of mastery was only five consecutive errorless trials. Animals not reaching the criterion in 75 trials proceeded to the next task after another three-day rest period. Total time, time in error, error, and trial scores were recorded. Only entrances (two-thirds) into culs were counted as errors. Time spent in the culs was the time-in-error score. Total time included this plus the time in the correct pathway.

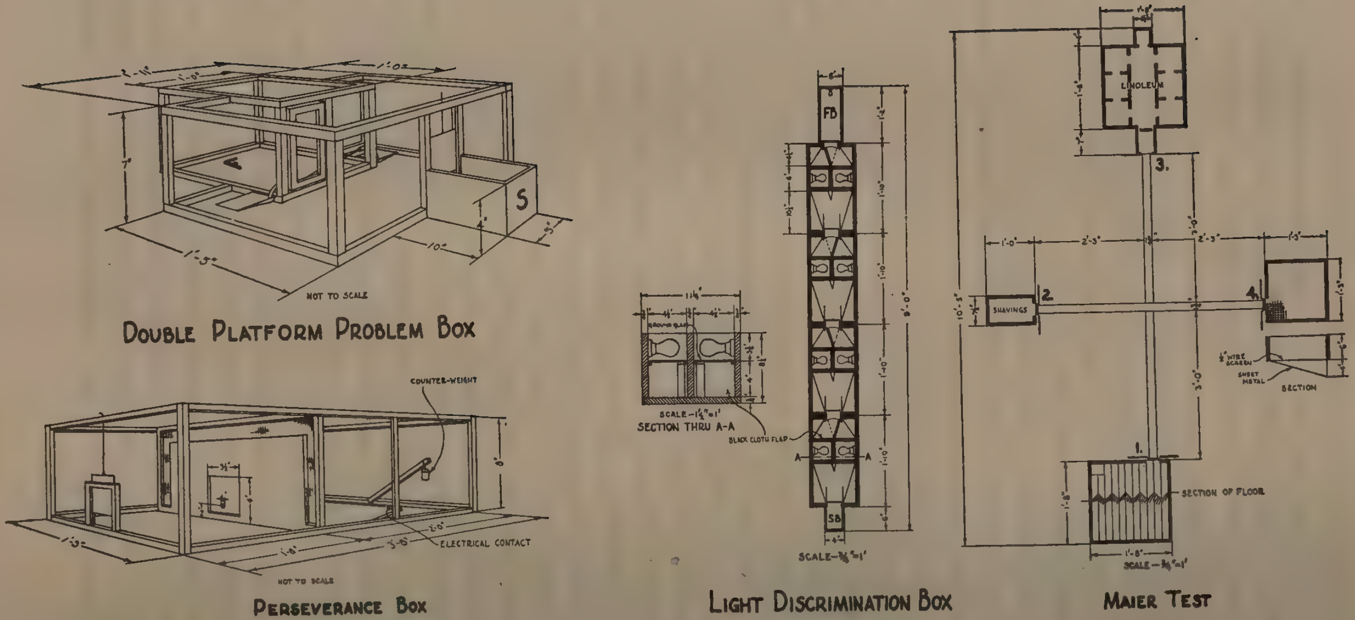
Rectangular maze. This apparatus is the same as Lashley's (6) Maze III and Lashley and Wiley's (8) Maze V. According to their results, error scores from it correlate highest of any variable studied with extent of cerebral lesion. Lashley (6) interprets it as a measure of intelligence of the rat. Figure 2 illustrates the design. The procedure was similar to that in the preceding tasks

—three days of preliminary feeding, 15 minutes a day, one trial on the fourth day, and ten trials a day thereafter until the animal had made either ten successive errorless runs or had taken a total of one hundred trials. Entrances into culs were recorded as errors, time spent in culs as time in errors, and time between complete exit from the starting box and two thirds' entrance into the food box as total time. The animals were given a seven-minute supplementary feeding period each day. The three-day rest period also followed this task.

Double platform problem box. Figure 3 gives a perspective of the problem box used. It is practically the same as Lashley's (6). To release the catch on the door leading to food, the animals had to push first on the right then on the left pedal. The release pedals required more than fifty and less than one hundred grams for operation. What the test measures is difficult to say, Lashley's (6, 7) results showing that it is not made more difficult by cerebral lesion as other latch boxes are. However, time scores give a plot in the nature of a learning curve.

Three days of preliminary feeding in the food compartment of this apparatus followed the rest period. On the fourth day the animals were given one trial. They were then given ten trials a day until mastery. The mastery criterion was five consecutive runs averaging five seconds or less per run, with none over eight seconds. Animals not reaching the criterion in 150 trials were discontinued on the problem. Supplementary feeding lasted for seven minutes. Total time and trial scores were recorded. Time, as before, was taken with the stop watch, and consisted of that elapsing between complete entrance into the reaction compartment and complete entrance into the food box. Three days of rest followed, as before.

Light discrimination box. Eighth in the series of tests was the Multiple-Light Discrimination Box. It is the same as that used by Krechevsky (5) and consisted of four units, in each of which the animal had to go through the light to reach the next. Figure 3 gives the design. The food reward was in a box at the end. The doors in the four sections could be operated with the lights to vary the order of correct runways and prevent solutions in



DOUBLE PLATFORM PROBLEM BOX

PERSEVERANCE BOX

LIGHT DISCRIMINATION BOX

MAIER TEST

FIG. 3. DIAGRAMS OF APPARATUS

terms of spatial cues. The order of correct pathways was altered each trial. No shock was used. Performance scores in this box were thought to measure a visual capacity of the animal, and it was included to enable identification of such. With the normal animal many other factors obviously enter.

One trial followed the three days of preliminary feeding and ten trials a day thereafter until mastery. The criterion was four out of five errorless trials with only one error on the unsuccessful trial. Animals not reaching the criterion in 70 trials were started on the next test. Supplementary feeding was six minutes. Time, trial, and error scores were recorded. Time scores were those between complete exit from the starting box and complete entrance into the food box. Entrances into the dark alleys were considered errors.

Maier test. Immediately following completion of the light discrimination task, each animal was placed on the modified Maier (12) apparatus, supposedly a test of reasoning ability of the rat. The apparatus used consisted of four boxes, of different sizes and floor texture, connected with each other by narrow strips of wood. The floor of one box was covered with shavings, another with linoleum, another with one-half inch wire screen, and the fourth with corrugated wood. The tops of the boxes were covered with white cotton cloth and their fronts with tin. Figure 3 includes an illustration of the apparatus.

The rats were allowed a six-day exploratory period on the apparatus, one hour each day. They were then given three trials a day for 15 days. The animal was fed for one minute in the correct box, placed in another, and allowed to run until it had reached the correct one. It was permitted to feed there for one minute and then taken to the next for exploration and feeding. A definite order of correct boxes for the 45 trials was maintained. On each trial the animal was started from the box which was correct for the preceding one. In making up the order of correct boxes, the experimenter kept three items in mind: (1) no box was to be correct twice the same day; (2) the box not correct the preceding day (there were four boxes and only three trials a day) was to be correct the first trial on the following; and (3) the correct box for one trial was to be the starting box for the next. Error scores (entrances into wrong boxes) were kept for each trial.

Only the last 43 animals were given this test. This fact was due to the writer's failure on an apparatus similar to Honzik's (4) to secure more than chance results with small dispersion, for the first animals tested. Inasmuch as only the perseverance test followed this one, it was not thought that this change would affect the results. The number of test periods and the number of trials per day on the Honzik and Maier apparatus were the same.

Perseverance box. The so-called Perseverance, or Persistence, Box was merely a compartment divided into two sections by a partition with a doorway. Varying thicknesses of paper could be inserted in the doorway to obstruct the animal's passage from the release compartment into the food box. Directly in front of the food was a falling platform, which the animal had to step on before eating. The platform was attached to an electric clock which stopped when the pedal dropped. When the animal was released, the clock was started; when it reached food, the clock stopped automatically. Figure 3 gives the design of the apparatus.

The animals were fed for three days, fifteen minutes a day, in the box. They were then placed in the outer compartment and had to find their way to the food. On the fourth day a sheet of 16-pound wood pulp paper was inserted between the two sections and the animal placed in the release compartment. A small hole near the center of the paper facilitated the animal's attack. On the days following, five, 18, and 32 sheets of paper were inserted in the slot. Animals not tearing through the obstruction in two hours were returned to their home cages without food. On the following day the next test was given.

It was thought that this test would measure a persistence element of the rats' behavior. Observation of the animals, however, showed that the reaction to the first obstruction was more one of startle, or surprise; although after having gone through the partition once or twice, the animals attacked the obstruction quite vigorously upon first being placed in the reaction compartment. Scores from this latter behavior (with 18 and 32 sheets) are what one might consider to be measures of persistence and those from the first type (on one and five sheets), measures of what Lashley (7) terms readiness with which the animals use force to meet an obstacle.

III. VARIABLES AND INTERCORRELATIONS

Measurements of different aspects of the rats' behavior in the twelve situations, combined with age, total percentage weight change before the beginning of each task and a second wildness measure, gave a set of 34 variables. In general four measurements were taken to represent the rat's performance in each situation. They were the number of entrances into culs-de-sac (errors) on a given number of trials, the number of trials to meet a criterion, the time spent in the correct pathway for a given number of trials, and the time per error for those trials. Product-moment correlations between these variables were computed.

Variables. The measures intercorrelated were as follows.

Variable 1: Age. The variables for each test were correlated with the animals' ages at the beginning of the task.

Variable 2: Percentage weight change. Total percentage weight change between the time that the animals were brought from the colony and the day they began each task was utilized for this variable. Since the incentive through the experiment was food, it was thought this measure would give a rough indication of different degrees of motivation. Stone (14) and others have used a similar measure to indicate varying degrees of hunger; however, under certain conditions (see variable 3) it was thought the weight change variable is predominantly a measure of other factors.

Variable 3: Wildness—percentage weight drop during the first three days after the animals were taken from the colony. The writer noticed that during these three days some of the rats refused to eat, and they urinated and defecated excessively. Stone (16), Yerkes (22), and others have used urination and defecation as criteria of wildness. Both refusal of food and excessive urination and defecation would tend to produce a weight drop, and the assumption made in terming this measure "wildness" is that the wilder animals tend to lose the most weight. Obviously, other factors enter to produce the weight drop.

Variable 4: Wildness—time in the stovepipe tunnel.

Variable 5: Activity—number of revolutions on the activity wheels, days 9–14. That this variable does not measure all types of activity is obvious.

Variable 6: Time in Straightaway, trials 15–20.

Variable 7: Trials to master first right-going habit.

Variable 8: Number of errors, trials 2–12, first right-going habit.

Variable 9: Time per error, trials 2–12, first right-going habit. (Time in errors divided by number of errors.)

Variable 10: Time in the correct pathway, trials 2–12, first right-going habit. (Total time minus time spent in errors.)

Variable 11: Trials to master left-going habit.

Variable 12: Errors, left-going habit, trials 2–16.

Variable 13: Time per error, trials 2–16, left-going habit.

Variable 14: Time in correct pathway, trials 2–16, left-going habit.

Variable 15: Trials to meet criterion, second right-going habit.

Variable 16: Number of errors, trials 2–13, second right-going habit.

Variable 17: Time per error, trials 2–13, second right-going habit.

Variable 18: Time in correct pathway, trials 2–13, second right-going habit.

Variable 19: Trials to meet criterion, Multiple-T Maze.

Variable 20: Errors, trials 2–16, Multiple-T Maze.

Variable 21: Time per error, trials 2–16, Multiple-T Maze.

Variable 22: Time in correct pathway, trials 2–16, Multiple-T Maze.

Variable 23: Trials to meet criterion, Rectangular Maze.

Variable 24: Errors, trials 2–12, Rectangular Maze.

Variable 25: Time per error, trials 2–12, Rectangular Maze.

Variable 26: Time in correct pathway, trials 2–12, Rectangular Maze.

Variable 27: Trials to meet criterion, Double-Platform Problem Box.

Variable 28: Time, trials 2–26, Double-Platform Problem Box.

Variable 29: Trials to meet criterion, Multiple-Light Discrimination Box.

Variable 30: Errors, trials 1–11, Multiple-Light Discrimination Box.

Variable 31: Time, trials 1–11, Multiple-Light Discrimination Box. (Time in errors not excluded.)

Variable 32: Number of errors, 45 trials, Maier Test.[1]

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[1] Errors and errorless trials on this test correlated −.74.

Variable 33: Total time to gnaw through one plus five sheets, Perseverance Box.

Variable 34: Total time to gnaw through 18 plus 32 sheets, Perseverance Box.

Correlation method. Intercorrelations between these variables were computed by the product-moment formula. The scores were first grouped into ten class intervals on the basis of 75 cases and the intercorrelations computed between variables 1, 2, 3, 4, 5, 7, 8, 11, 12, 15, 16, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 33, and 34. Records for rat 20 were lost on the Rectangular Maze and were not obtained for rats 27, 58, and 80 on the Perseverance Test. To facilitate computation and inclusion of their records on the other tests, the median class interval scores for the group on these tasks, were listed for these animals. Intercorrelations with variables 6, 9, 10, 13, 14, 17, and 18 were based on 63 cases, and those with 32, on 43 cases. Regroupings for those 43 animals were made on the variables in which the number of class intervals represented was less than six. The cross product, squares, and sums were computed by the Tabulating Bureau of the University. The correlations were made to three places but rounded off to two for publication purposes.

Intercorrelations. Table II gives the intercorrelations between the 34 variables. In general they are low. About 30 per cent have an absolute value equal to or greater than .20. Only 22 of the 561 correlations are greater than .40. These small values are in accord with those ordinarily found in animal studies, as indicated by Thorndike's (17) review.

IV. FACTOR ANALYSIS

The problem set for this experiment required:

(1) the factorial description of the variables in terms of a set of arbitrary reference vectors; and

(2) the rotation of the reference axes towards the clusters of variables.

Thurstone's (18) centroid method with unknown diagonals was used to determine the arbitrary reference axes. An attempt to discover a simple order in the configuration was made with an

Intercorrela

VARIABLE NUMBER	1	2	3*	4	5	6	7	8	9	10	11	12	13	14	15	
1																
2	*															
3	−.45	†														
4	−.02	−.21	+.21													
5	−.09	−.33	+.06	−.07												
6	−.13	+.06	−.03	+.10	−.13											
7	+.10	+.13	−.19	+.09	−.14	+.22										
8	+.17	+.02	−.33	+.02	+.08	+.17	+.29									
9	−.11	+.12	+.01	+.01	−.11	+.56	+.22	−.01								
10	−.06	+.02	+.12	+.05	−.05	+.38	+.28	+.44	+.27							
11	+.18	+.21	−.23	−.07	+.01	+.08	+.34	+.20	+.13	+.23						
12	+.07	+.16	+.17	+.19	−.01	−.07	+.12	+.06	+.01	+.10	+.05					
13	+.09	+.22	−.09	+.08	−.22	+.31	+.14	−.09	+.53	+.30	+.19	+.06				
14	−.14	+.25	+.15	+.29	−.13	+.27	+.10	+.05	+.11	+.53	+.11	+.32	+.33			
15	+.25	+.36	−.05	−.02	+.05	−.07	+.32	+.28	+.01	+.15	+.20	+.31	+.01	+.00		
16	+.14	+.15	+.01	−.25	+.10	−.10	+.15	+.10	−.18	−.16	+.07	+.10	−.27	−.10	+.20	
17	−.20	+.09	+.08	−.05	−.25	+.02	−.11	−.31	+.06	−.01	−.14	+.07	+.31	+.27	−.20	−
18	−.10	−.19	+.10	−.07	+.17	+.05	−.18	−.01	−.09	−.15	−.06	−.23	+.01	−.11	−.19	−
19	+.27	+.10	−.08	−.01	−.16	+.18	+.10	+.08	+.18	−.08	+.25	+.20	+.12	+.10	+.12	+
20	+.07	+.02	−.09	−.11	−.20	+.18	+.00	+.11	−.05	−.16	+.13	+.05	−.05	+.02	+.09	+
21	+.00	+.11	+.01	−.08	−.04	+.46	+.05	+.14	+.33	+.33	+.21	+.20	+.31	+.33	+.03	−
22	+.22	+.12	−.20	−.13	−.12	+.47	+.10	+.15	+.19	+.18	+.20	+.01	+.28	+.27	−.01	+
23	+.28	+.25	−.18	−.13	−.04	+.21	+.43	+.26	+.19	+.14	+.44	+.28	+.19	−.04	+.29	−
24	+.34	+.24	−.09	−.09	+.12	−.01	+.23	+.24	−.05	+.07	+.30	+.16	+.02	−.01	+.15	+
25	+.27	+.19	−.01	+.09	+.17	+.04	+.00	+.01	−.04	+.05	+.13	+.04	+.21	+.13	−.04	−
26	+.33	+.17	−.04	+.04	−.00	+.15	+.10	+.04	+.03	+.04	+.20	+.15	+.18	+.15	−.02	−
27	−.04	+.19	−.14	−.12	−.04	+.22	+.11	+.06	+.08	+.25	+.17	+.29	+.22	+.28	+.27	−
28	+.24	+.07	−.10	−.20	−.15	+.32	+.03	+.12	+.22	+.02	+.19	+.20	+.17	+.03	+.06	−
29	+.24	+.10	−.24	−.06	−.06	+.11	+.18	+.23	+.06	+.13	+.28	+.16	+.19	+.05	+.12	+
30	+.10	+.11	−.19	+.02	+.01	+.15	+.14	+.15	+.16	+.15	+.27	+.08	+.31	+.08	+.07	−
31	−.06	+.15	−.10	−.08	−.07	+.37	+.09	+.03	+.20	+.14	+.28	+.06	+.32	+.08	+.09	−
32	−.03	+.23	−.02	+.04	+.18	+.07	+.32	+.25	+.02	+.17	+.03	−.27	−.27	+.15	+.05	+
33	−.06	+.00	+.13	−.14	+.18	−.03	+.06	+.05	−.14	−.09	−.05	+.10	−.18	−.13	−.10	+
34	−.16	+.17	−.24	+.02	−.08	+ 04	−.02	+ 24	+.11	+.09	−.03	−.16	+.21	−.01	+.05	−

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* Correlations between 1 and 2 were different for each task. All but two were less than .20, and

† This correlation is in a sense a reliability coefficient, and the highest correlation of the two v

E II

tween variables

	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
6																	
4	−.14																
2	−.13	+.58															
0	+.07	+.26	+.50														
7	+.05	+.36	+.74	+.76													
5	−.03	+.22	+.21	+.26	+.26												
4	+.07	+.22	+.25	+.13	+.19	+.28											
0	−.05	+.04	+.06	+.23	+.20	+.15	+.53										
4	−.03	+.11	+.21	+.22	+.23	+.18	+.74	+.74									
7	+.06	+.13	+.21	+.40	+.32	+.35	−.01	+.10	+.06								
0	−.10	+.28	+.38	+.41	+.46	+.27	+.19	+.20	+.37	+.32							
2	+.07	+.18	+.15	+.05	+.18	+.38	+.15	−.02	+.03	+.36	+.15						
5	+.06	+.28	+.24	+.24	+.33	+.34	+.10	+.15	+.02	+.22	+.23	+.38					
5	−.01	+.32	+.14	+.27	+.25	+.23	+.09	+.23	+.16	+.25	+.41	+.11	+.68				
1	+.17	−.11	−.14	−.13	−.05	+.00	+.31	−.02	−.01	−.09	+.08	+.04	−.05	+.00			
4	+.05	−.17	−.14	−.10	−.13	−.05	+.11	−.09	−.02	−.11	+.12	−.03	−.25	−.11	+.35		
8	−.02	+.07	+.18	+.15	+.18	+.04	−.12	−.13	−.04	+.10	+.25	+.11	+.07	−.04	+.04	−.03	

ry was made zero for analysis.

(taken with negative sign) was entered for the factor analysis.

unpublished graphical method devised by Thurstone for configurations in more than three dimensions. Only orthogonal rotations were made. Although there is a possibility that oblique rotations would give results more adequately satisfying the criteria for a simple structure, it was thought that the factors obtained by the orthogonal rotations would closely approximate those which would be otained by the oblique transformations. It might be stated that the axes have remained practically orthogonal in several studies in which oblique transformations have been made.

Centroid Analysis

Centroid matrix. A matrix with eight columns, or factors, was obtained by the centroid method. Table III is this matrix with the communalities. The centroid analysis was continued until the greatest contribution that any loading could make to the communalities was equal to or less than .10. Two exceptions were permitted (on variables 6 and 32), as the loadings were but .36; and their contributions to the communalities were but .13. Two more factors were added after this analysis had been made, to insure that sufficient degrees of freedom were allowed for the rotations.

Residuals. Figure 4 is the distribution of the eighth factor residual coefficients. About 87 per cent of them are equal to or smaller than .10. None is greater than .30.

Communalities. The communalities of the variables are in general low. The median is about .49.[2] Five variables have communalities greater than .70. They are numbers 20, 22, 24, 25, and 26. The variance in scores on these tests is pretty well accounted for by factors common to several variables. Seven variables—numbers 4, 5, 12, 16, 20, 33, and 34—have communalities lower than .30. This fact indicates that these variables measure little in common with others of the battery.

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[2] The reliability of the tests would be higher, as the communality does not include that part of the variance in scores which is specific to a particular test.

TABLE III

*Centroid matrix and communalities**

	I	II	III	IV	V	VI	VII	VIII	h^2†
1	+.27	−.43	+.36	+.16	−.22	−.13	−.09	+.20	.53
2	+.33	+.24	+.18	−.08	−.13	−.32	+.17	−.37	.50
3	−.22	+.32	−.09	−.32	−.31	−.23	+.24	−.36	.59
4	+.02	+.20	−.05	−.17	+.14	−.28	+.19	+.26	.27
5	−.23	−.11	+.19	−.11	−.15	+.30	−.15	−.05	.24
6	+.47	+.24	−.28	−.06	−.07	+.36	+.10	+.21	.56
7	+.40	+.21	+.41	+.05	−.21	+.15	+.18	+.04	.48
8	+.33	+.08	+.39	+.20	+.09	+.24	+.16	+.24	.45
9	+.36	+.30	−.22	−.12	+.19	+.16	−.07	+.10	.38
10	+.42	+.48	+.12	−.16	−.07	+.18	+.14	+.30	.60
11	+.45	−.10	+.26	−.00	+.18	+.19	+.01	−.09	.36
12	+.26	+.12	+.13	+.19	+.03	−.30	−.07	−.25	.29
13	+.51	+.23	−.27	−.24	+.26	−.09	−.29	+.09	.61
14	+.39	+.44	−.11	−.26	−.26	−.15	+.15	+.04	.53
15	+.28	+.08	+.39	+.24	−.08	−.13	+.16	−.15	.37
16	−.06	−.19	+.37	+.12	+.22	+.09	+.33	−.25	.42
17	+.15	−.02	−.36	−.31	−.11	−.25	−.08	−.15	.35
18	−.12	+.05	−.15	−.15	−.14	+.24	−.20	−.15	.20
19	+.48	−.28	−.20	+.19	−.06	−.16	+.18	−.22	.49
20	+.43	−.41	−.31	+.46	+.29	−.10	+.27	−.05	.83
21	+.60	+.03	−.34	+.11	+.38	+.15	−.10	+.11	.68
22	+.63	−.21	−.34	+.24	+.35	+.17	+.07	+.19	.81
23	+.56	+.03	+.28	+.18	−.17	+.11	−.22	−.22	.57
24	+.41	−.37	+.51	−.30	−.32	−.06	−.08	−.12	.78
25	+.38	−.37	+.13	−.34	−.23	−.18	−.21	+.09	.73
26	+.47	−.38	+.19	−.43	−.33	−.27	−.30	+.14	.87
27	+.45	+.18	+.03	+.21	+.10	+.08	−.18	−.22	.38
28	+.53	−.16	−.13	+.19	+.24	+.09	−.23	−.10	.48
29	+.38	+.02	+.14	+.19	−.15	+.15	−.12	−.18	.29
30	+.52	−.16	−.17	−.10	+.31	+.29	+.12	−.27	.59
31	+.50	−.16	−.26	−.26	+.17	+.31	+.12	−.35	.68
32	−.00	+.13	+.34	−.15	−.29	+.36	+.25	−.09	.44
33	−.21	+.05	+.28	+.08	+.31	+.21	−.11	−.11	.29
34	+.21	+.19	−.16	+.19	+.04	−.04	−.10	+.08	.17

* Computations were carried out to three places but numbers rounded off to two for publication.

† Communalities.

Rotation of axes

The centroid matrix was simplified by rotating the reference axes in orthogonal planes to maximize the number of zero entries in the table. This procedure not only increases the number of zeros; but it also increases the projections of some of the points on the reference axes. It reduces the matrix to its simplest possible order with orthogonal reference axes.

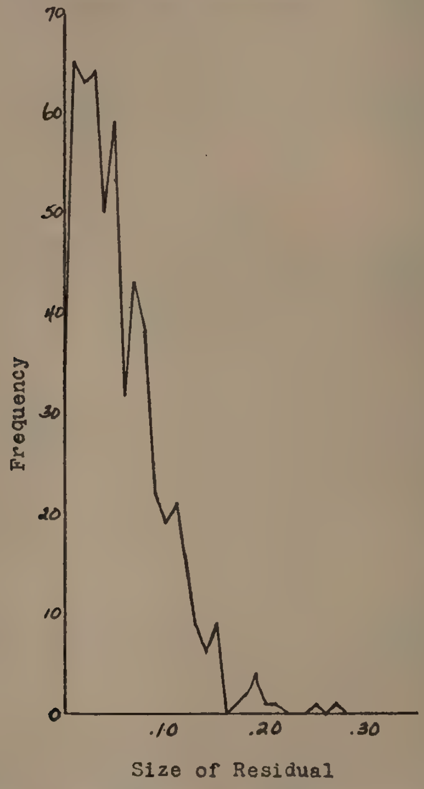

FIG. 4. DISTRIBUTION OF EIGHTH FACTOR RESIDUAL COEFFICIENTS

Method. Plots of each factor with every other one were made. For instance, the entries in column I of the centroid matrix were plotted against the entries in II, against those in III, in IV, and so on, successively. Those in II were also plotted against III, IV, V, VI, and so on. Likewise columns III, IV, and V were plotted against the remaining ones. Twenty-eight such graphs were thus obtained. Figures 5, 6, 7, 8, 9, and 10, represent the type

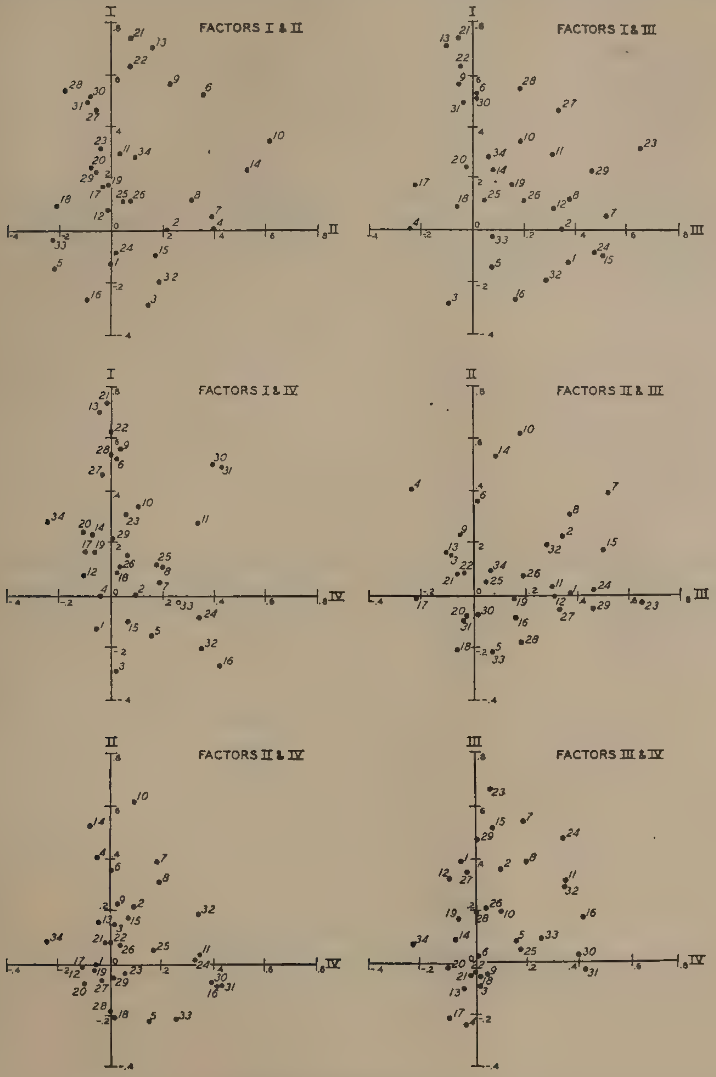

FIG. 5. FACTOR PLOTS

of graph made; however, they are plots of the final factorial matrix.

By inspection, new orthogonal axes were drawn through clusters of points in such a manner that the projections of the points

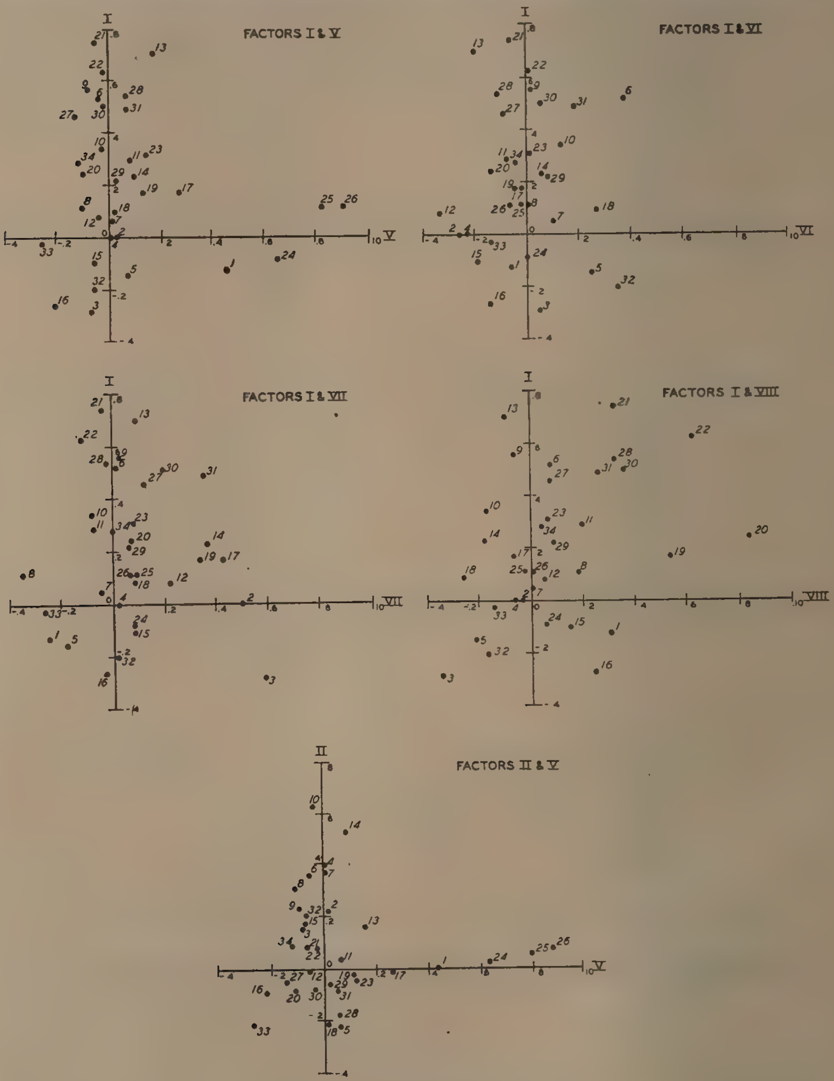

Fig. 6. Factor Plots

upon these axes would in the greatest number of instances be zero:[3] that is, the axes were passed through clusters of the variables. A transformation matrix by which the centroid matrix was to be multiplied was then set up. The transformation

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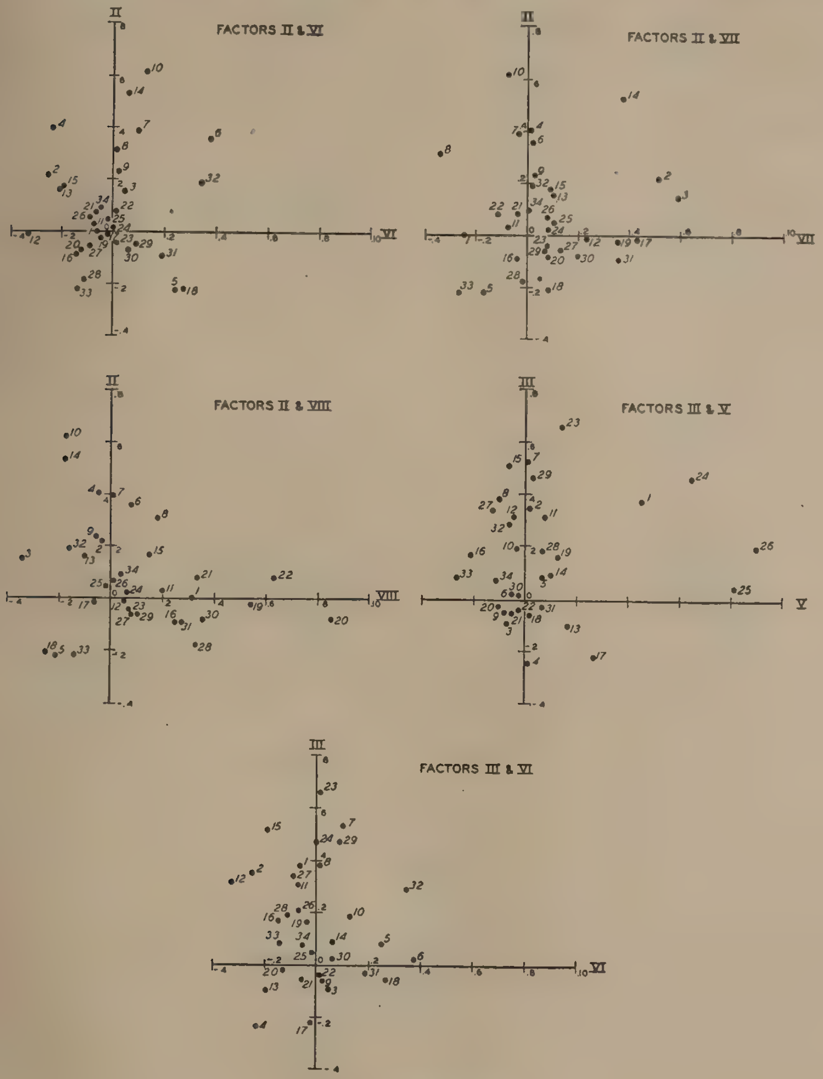
[3] Loadings between ±.10 were considered zero within experimental error.



FACTORS II & VI

FACTORS II & VII

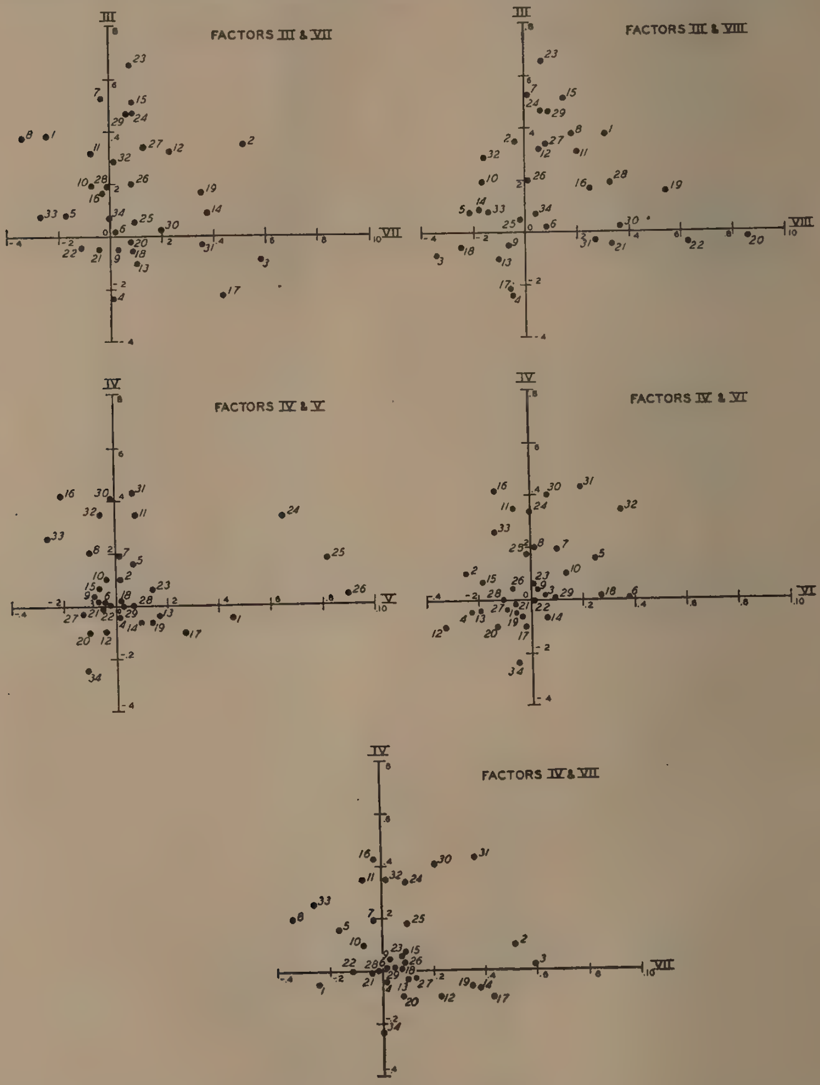
FACTORS II & VIII

FACTORS III & V

FACTORS III & VI

Fig. 7. Factor Plots

matrix was one which, by the multiplication, would rotate the reference axes to the positions desired, as indicated by inspection of the graphs. The sine and cosine of the angles of rotation, entered in the transformation matrix, were taken from the graphs.


FACTORS III & VII

FACTORS III & VIII

FACTORS IV & V

FACTORS IV & VI

FACTORS IV & VII

Fig. 8. Factor Plots

The procedure was repeated for the factorial matrix obtained by the first rotation. That is, graphs were made, a second transformation matrix set up, and a third matrix obtained. In all, 13 such rotations were made. Table IV is the composite orthogonal transformation matrix. The centroid matrix multiplied by this one reproduces the factorial matrix.

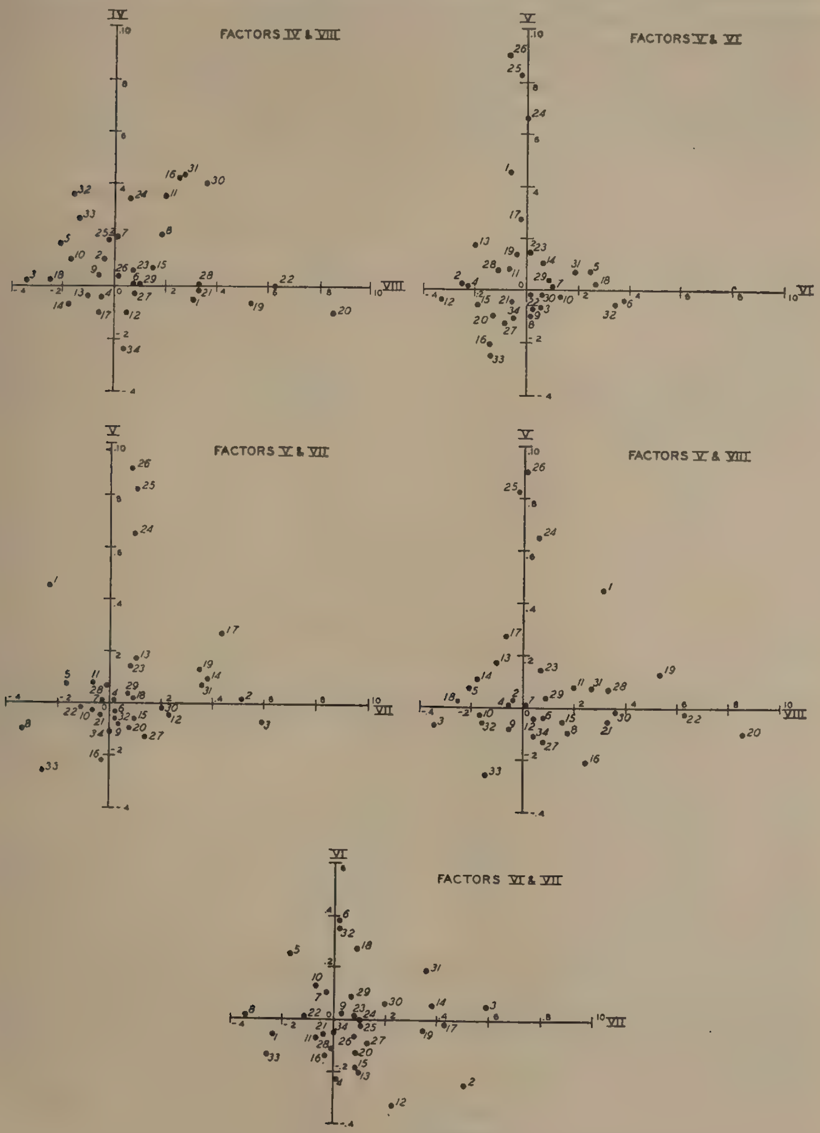


Fig. 9. Factor Plots

Factorial matrix. Table V is the final factorial matrix. Figures 5, 6, 7, 8, 9, and 10 are plots of this matrix. The plot of I against II, for instance, indicates that Factor I is markedly manifest in Variables 21, 13, and 22, and Factor II is absent. Likewise the plot of I against III shows variables 21 and 22 to be highly loaded with I but not with III, and so on. The factor plots also show that only very slight rotations which will gain zeros can be made by this method, with the exception of the plots of I against VIII and II against VI. These axes may be rotated somewhat, but the interpretation of the loadings as they are is better than they would be with the change. The rotations were made, however, to discover whether they would provide for further transformations on other axes, but they do not.

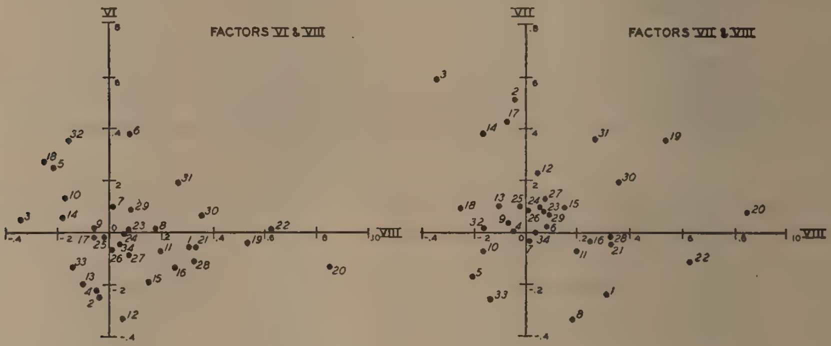


FIG. 10. FACTOR PLOTS

TABLE IV

Transformation matrix

	1	2	3	4	5	6	7	8
1	+.66	+.32	+.43	+.10	+.30	−.06	+.20	+.37
2	+.23	+.47	+.13	−.16	−.52	−.08	+.15	−.61
3	−.38	+.11	+.62	+.44	+.11	−.30	−.36	−.19
4	−.07	−.18	+.42	−.52	−.51	−.05	−.23	+.45
5	+.35	−.14	−.36	+.38	−.36	−.59	−.28	+.16
6	+.27	−.17	+.10	+.41	−.28	+.72	−.33	−.05
7	−.40	+.54	−.15	+.33	−.32	+.14	+.26	+.47
8	+.03	+.53	−.27	−.29	+.24	+.08	−.70	+.05

TABLE V

*Factorial matrix**

	I	II	III	IV	V	VI	VII	VIII
1	−.13	.00	+.38	−.05	+.45	−.06	−.24	+.31
2	.00	+.22	+.35	+.10	+.02	−.25	+.51	−.04
3	−.29	+.15	−.09	+.02	−.07	+.05	+.59	−.34
4	.00	+.40	−.24	−.04	+.01	−.23	+.01	−.05
5	−.15	−.22	+.08	+.16	+.07	+.25	−.17	−.21
6	+.52	+.36	+.02	+.01	−.04	+.38	+.02	+.08
7	+.05	+.39	+.53	+.19	+.01	+.10	−.03	+.01
8	+.11	+.31	+.38	+.20	−.10	+.01	−.34	+.18
9	+.56	+.23	−.05	+.04	−.08	+.02	+.03	−.06
10	+.34	+.62	+.19	+.10	−.03	+.13	−.07	−.17
11	+.29	+.03	+.31	+.35	+.08	−.07	−.07	+.20
12	+.08	−.01	+.32	−.10	−.04	−.33	+.23	+.05
13	+.70	+.16	−.10	−.04	+.17	−.20	+.10	−.10
14	+.23	+.53	+.09	−.07	+.10	+.06	+.38	−.18
15	−.10	+.17	+.51	+.07	−.06	−.19	+.09	+.15
16	−.27	−.09	+.17	+.42	−.21	−.14	−.03	+.25
17	+.17	−.01	−.22	−.10	+.27	−.02	+.43	−.06
18	+.09	−.21	−.06	+.02	+.02	+.27	+.09	−.25
19	+.17	−.02	+.16	+.06	+.13	−.04	+.35	+.54
20	+.24	−.08	−.02	−.10	−.10	−.13	+.08	+.85
21	+.74	+.08	−.05	−.01	−.05	−.06	−.04	+.33
22	+.63	+.08	−.04	.00	−.02	+.01	−.11	+.63
23	+.31	−.04	+.66	+.06	+.14	+.01	+.08	+.07
24	−.09	+.02	+.47	+.34	+.65	.00	+.09	+.06
25	+.11	+.05	+.05	+.18	+.82	−.02	+.10	−.02
26	+.11	+.07	+.20	+.04	+.90	−.07	+.08	+.01
27	+.46	−.06	+.34	−.03	−.13	−.09	+.13	+.08
28	+.54	−.18	+.19	.00	+.07	−.11	−.01	+.33
29	+.22	−.06	+.47	+.01	+.03	+.09	+.07	+.09
30	+.50	−.08	+.02	+.40	−.02	+.06	+.20	+.36
31	+.49	−.09	−.03	+.43	+.07	+.19	+.36	+.27
32	−.20	+.19	+.29	+.35	−.06	+.35	+.02	−.16
33	−.03	−.22	+.08	+.26	−.26	−.14	−.26	−.14
34	+.28	+.09	+.07	−.24	−.11	−.05	.00	+.04

* The term "factorial matrix" ordinarily denotes any matrix which when multiplied by its transpose reproduces the correlational matrix. It is used herein to designate that matrix which best satisfies the definition of simple structure by the method used.

V. DISCUSSION

The problem set for investigation requires the answer to two questions:

(1) To what extent are the various factors involved in the variables selected for study? and

(2) What do the factors mean psychologically?

The loadings given in Table V answer the first question. The second question requires an interpretation of the factors. The diagrams in Figure 5, that is, the plots of the first four factors, indicate that the variables lie in fairly definite clusters about the I, II, III, and IV axes; and the loadings with those factors show fairly definite trends in terms of psychological categories; however, some of the loadings seem to be non-meaningful. This fact may be due to chance elements, to inadequate rotational methods, or to inadequacy of the interpretations, which seem to be most plausible. Until further work has been done with a larger number of tests in each category, however, it seems advisable to consider the apparent exceptions as due to chance.

TABLE VI

Expected and actual weightings of variables if I is general speed factor

VARIABLES WITH EXPECTED LOW WEIGHTINGS	ACTUAL WEIGHTINGS	VARIABLES WITH EXPECTED HIGH WEIGHTINGS	ACTUAL WEIGHTINGS
7	+.05	6	+.52
8	+.11	9	+.56
11	+.29	10	+.34
12	+.08	13	+.70
15	−.10	14	+.23
16	−.27	17	+.17
19	+.17	18	+.09
20	+.24	21	+.74
23	+.31	22	+.63
24	−.09	25	+.11
29	+.22	26	+.11
30	+.50	27	+.46
32	−.20	28	+.54
		31	+.49
		33	−.03
		34	+.28

Identification of factors

Factor I is clearly a speed factor of some sort. The high loadings from as diverse measures as time scores on the Multiple-T

Maze and Double Platform Problem Box would indicate that the factor is quite general to motor tasks. It is perhaps the trait that Davis and Tolman (1) have distinguished as "slap-dash vs. deliberate." Assuming that such a factor is general to all motor tasks, one would expect high and low loadings as designated in Table VI. The actual loadings are given beside the numbers of the variables. Loadings of .30 or less might be termed insignificant.

There are but two variables which should have low weightings but which have loadings above .30, viz., Variables 23 and 30. The loading of Variable 30 is .50 but may be attributed to the close relationship between total time (time in errors not partialed out on this test) and number of errors. The addition of another factor would probably remove this loading, but the additional factor would be defined by only two variables. The loading of Variable 23 is but .51. Hence, variables other than time scores do not seem to measure the factor.

Of the 16 variables which should have high loadings according to this interpretation, seven have weightings below .30. They are Variables 14, 17, 18, 25, 26, 33, and 34. Variables 25 and 26 (from the Rectangular Maze) were apparently affected by some highly specific factor introduced at the time of this test, as they are loaded unusually high with Factor V. Other exceptions may have somewhat similar explanations. The results are quite suggestive, and the factor is one which should be explored further.

Factor II is a wildness-timidity trait. Arguments sustaining this interpretation are: (1) time in the Wildness Tunnel (Variable 4) has a relatively high loading; (2) tests in the earlier part of the series have high loadings, and those in the latter part have zero loadings; and (3) variables which would be most affected by hesitancy in entering trap doors (time in the correct pathway) during the earlier part of the training series have the highest loadings. Loadings as small as .25 are meaningful. Smaller ones may have some significance.

Factor III is interesting. Trial scores on the simpler tasks are consistently heavily loaded. Error scores on these tasks are weighted in the same direction but in general less highly. The loadings of time scores are negligible. Variables 1 and 2 (age and percentage weight change) are loaded above .30 with III, but they are rough measures of the number of trials taken to master a previous task. That is, a group of animals that took a relatively large number of trials to master one task would in turn be slightly older and would gain slightly more weight than another group that took fewer trials. One would be tempted to term the factor a motivational one were it not for this fact, or if percentage weight change were loaded highly but not age. It may be, however, that the loading of the age variable has an explanation similar to that of Variable 30 with Factor I. If so, the factor might be termed "motivation," but then it is difficult to explain the low loadings of the time variables: supposedly differences in motivation would be reflected in time scores as much as in error or trial scores.

It is more than likely that the factor is some sort of memory or learning ability. The Maier test might be considered a measure of immediate memory, and it has a fairly high loading. Assuming that learning involves the repetition of correct responses more than the elimination of errors, the higher loadings of the trial variables would be explained. This interpretation—that is, of associative learning—seems most probable. On the other hand, it is difficult to explain the low loadings of the variables from the Multiple-T Maze. However, this task may involve a higher intellectual ability. It might be pointed out that the tasks which define III are probably simple enough for the rat that they can be learned in terms of insight into the situation. A simple principle—different in each—runs through all of the tasks. In one instance, the principle is a right-going one, in another a left-going one, in another a right-left alternation, and so on. There is some evidence from Thorndike's (17) factor plots (of I against III) that mazes involving only a simple principle are more closely related than those which involve a more complex one.

Factor IV, although defined by a fairly definite cluster, is not clear cut psychologically. The axis is defined by the error and total time variables from the Multiple-Light-Discrimination Box, the error variable from the second reversal habit, the trial variable from the first reversal habit, errors on the Rectangular

Maze, errors from the modified Maier test, and time to reach food in the first obstruction test. It seems likely that the loadings are due to a transfer effect: the reversal habits are on the apparatus which was used to set up the original right-going response, and the other tests with relatively high loadings were late in the training series. Considering the high loadings of the variables from the light discrimination apparatus, one might be led to believe that the factor is a visual one. However, some unpublished extirpation work with the Multiple-T Maze used in the experiments indicate that a visual element was involved in its mastery, but it has no loading with IV.

Factors V, VI, VII, and VIII seemingly cannot be identified. Factor V is quite conspicuous, but the loadings are restricted to variables from the Rectangular Maze. Factor VIII is similarly defined by variables from the Multiple-T Maze. Other than indicating the adequacy of the factor methods used (one would expect highly specific factors for some of the apparatus), the loadings show little.

Factors involved in tests

Although the entries in Table V roughly indicate to what extent the various tests measure these factors, it seems desirable to summarize the findings in terms of the interpretations tentatively suggested. It must be borne in mind, however, that the loadings may be markedly influenced by the conditions of the experiments and by the population used. The general applicability of the conclusions should be tested in other situations.

Wildness tunnel. The time variable from this test seems to differentiate animals from a fairly tame stock on the basis of a wildness-timidity trait. The variable has small negative loadings with an associative or insight learning factor and with another factor which cannot be identified in this battery. The latter two loadings are probably insignificant. The communality of the variable in this battery is small. As used, it is probably not very reliable.

Activity cage. The type of activity measured in this apparatus has little relationship to that involved in other tests of the battery. That is, the communality of the variable is small. The variable has slight positive or negative loadings with all but Factors III and V. The diverse loadings may indicate that the activity variable measures a complex of factors, although some loadings are probably due to chance.

Straightaway. The time variable from the Straightaway measures a speed factor general to several tasks. In this battery it also measures the wildness-timidity trait and another not identified.

Simple maze. Three habits were set up on this maze: The original right-going one, a left-going habit, and a second right-going habit. The latter two are reversal habits. Trial, error, time per error, and time in the correct pathway are variables which were used in these experiments.

Trial variables on all three tasks involve an associative or insight learning factor. Error variables likewise measure this factor, with the exception of that on the second right-going habit. In addition, trial and error variables on the first right-going habit involve the wildness-timidity trait. One would not expect the latter trait to be involved in the test were the animals thoroughly tamed. They had been handled for a long period, however, before this task. The trial variable on the first reversal habit and the error variable on the second reversal habit involve a transfer factor.

Time scores on the first right-going and the left-going habits involve a speed factor. The wildness-timidity trait was markedly manifest in the time-in-correct-pathway variables of these two tasks. This latter fact is probably due to retracings in the correct pathway after the animal had reached the door at the end.

Multiple-T maze. The trial and error variables are specific to the apparatus in this battery. Time per error in the first few trials seems to be an excellent measure of the speed factor isolated. Besides its loading with I, the latter variable has a slight weighting with the factor specific to the apparatus. Time in the correct pathway measures the speed factor and the specific factor.

Rectangular maze. Trial and error scores from this maze, and especially the former, measure the associative or insight learning factor. Errors also measure the factor which, in this battery, is highly specific to the apparatus. The trial variable is loaded .31 with the speed factor, probably fortuitously. The two time variables measure only the factor specific to the apparatus.

Double platform problem box. The two variables, trials and time, from this task measure the speed factor. The loading of trials with I is undoubtedly due to the fact that the criterion of mastery is a function of the time taken to operate the latches on successive occasions. The trial variable also measures the associative or insight learning factor, and the time variable has a loading of .33 with Factor VIII.

Light discrimination box. The trial variable measures the associative or insight learning factor. Three factors seem to be involved in the error variable: the speed factor, the transfer factor, and VIII. The loading of the error variable with the speed factor is probably spurious. The time variable largely measures the speed and the transfer factors. The time variable also has a fairly large loading, .36, with Factor VII.

Modified Maier test. The error variable from this test has loadings greater than .15 with six of the eight factors. It thus seems to be a measure of a complex of factors, although some of the diverse loadings are probably due to chance. It has a relatively high loading with the transfer factor, and one equally large with VI. It has weighting of .29 with the associative or insight learning factor.

Obstruction box. The communalities of the variables from this apparatus are small. As used, the solution of the problem seems to be largely a matter of chance.

Summarizing, one finds only five variables which measure but one readily-identifiable factor, and their communalities are not extremely high. They are: (1) time in the Wildness Tunnel, which seems to measure the wildness-timidity trait; (2) time per error on the first right-going habit, which seems to measure the speed factor; (3) trials on the second right-going habit, which measure the associative or insight learning factor; (4) time per error on the Multiple-T Maze, which measures the speed factor;

and (5) trials to master the Rectangular Maze, which seem to measure the associative or insight learning factor. Three other variables are loaded with but one factor, but the factors with which they are loaded are not identifiable in this battery. Two or more factors seem to be involved in all other tests, or the communalities are so low that the tests may be considered unreliable.

Modifications of tests for further study

The results from this study point to the need for developing reliable tests of unitary variables. For further factor analysis work it is also necessary that several tests in each category be developed. It is quite probable that slight modifications of tests used in this battery will greatly increase their value.

Although the time variable on the Wildness Tunnel is a fair test of one of the factors isolated, the measure would be improved by less preliminary feeding in the food box. Three or four trials, instead of one, would probably increase its reliability. The scoring might be improved by recording the time spent in the Tunnel after the animal reaches the exit. In so doing, one would obtain even a better measure of its hesitancy in leaving the darkened "shelter." Another measure of this same trait would be the number of retracings after the animal had reached the exit. A short straightaway with buzzer and trap door at the end might also be used. Two scores would be obtained from this task; time to enter the food box and number of retracings after reaching the trap door. Several trials on this latter apparatus should give a fairly reliable measure of the wildness-timidity trait.

Tryon's (21) eight principles of measurement important for animal work need not be reviewed. However, preliminary training to depress the wildness trait should extend over a long period, as this trait seems to persist for some time. One might use norms from wildness-timidity tests as a basis for deciding when the animals are sufficiently test-broken to begin training on the "learning" tests. Increasing the length of tasks, as Tryon (21) points out, would also tend to increase their reliability, although it is quite possible that by lengthening tests used in the learning situation, one at the same time changes the essential nature of the tasks.

From the loadings with III in the present experiment, one would expect tasks involving only a simple principle to be closely related. A better control of motivation than is possible over a long training period would be advisable in determining definitely whether the factor is primarily a learning one. The addition of more similar elements to the rectangular maze, the light discrimination box, and other tests of this nature should increase their reliability. Similar tests in other sensory spheres should be correlated with ones in this battery to determine whether the trait is central. The type of learning measured may involve the repetition of correct responses more than the correction of errors, and it is possible that the scoring of such tasks in terms of the number of correct responses at the choice points would give a better measure of the ability.

Interestingly, the activity variable has but small loadings with the various factors, yet a certain amount of activity is involved in all tests. It may be that the period in the cages was too long, and samplings over a shorter period of time would give results which correlate higher with performance on mazes and other apparatus. Several activity tests should be included in further work to insure isolation of an activity trait. Persistence might be measured better by modifying the procedure with the paper obstruction box. The ratio of the amount of time spent in working at the obstruction to the amount of time in other activities during a two-hour period (after the response has been learned) might be considered such a measure. The animal should be given ten or fifteen trials first with only a few sheets of obstruction. Then the obstruction should be increased to 50 or 60 sheets.

Summary and conclusions

Intercorrelations between a set of 34 variables, derived from twelve tests commonly used with the rat, plus age and motivation variables were analyzed by Thurstone's centroid method of factor analysis and the reference axes rotated orthogonally to maximize the number of zeros in the factorial matrix. Eight factors adequately account for the intercorrelations. Four factors were tentatively named speed, wildness-timidity, associative or insight learning, and transfer. The other factors were left unidentified.

Many of the tests used involve several factors, or their communalities are so low that they may be considered unreliable. If the factors are to be more clearly isolated, each category must be explored by increasing the number of tests in that category. Modifications of the tests were suggested, which should improve their reliability and at the same time restrict the measurement to one category.

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APPENDIX

Figures 11 and 12 give the distributions of raw scores on 32 of the variables. The dotted lines indicate the frequency distributions after the first 12 cases had been removed, the omission of which was necessary on variables 3, 4, 5, 7, 8, 11, 12, 15, 16, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 33, and 34 for correlations with variables 6, 9, 10, 13, 14, 17, and 18. Correlations with variable 32 were based upon 43 cases, which fact required still further reduction of the distributions. However, these reduced ones are not shown. Their form is similar to the ones given. The distributions of variables 1 and 2 are likewise not shown, as they are somewhat different for each test, and the presentation would require 20 separate charts. The dispersion of variable 1 (age) is small.

The distributions of scores on the tests are interesting. Error scores seem to be more normally distributed than the others. In general, time scores show a bunching at the lower ranges, this fact probably indicating the approach to a physiological limit. Trial scores are, in general, more widely distributed and show a slight bunching at the upper limit. This fact is probably due to the upper limit set by the experimental procedures.

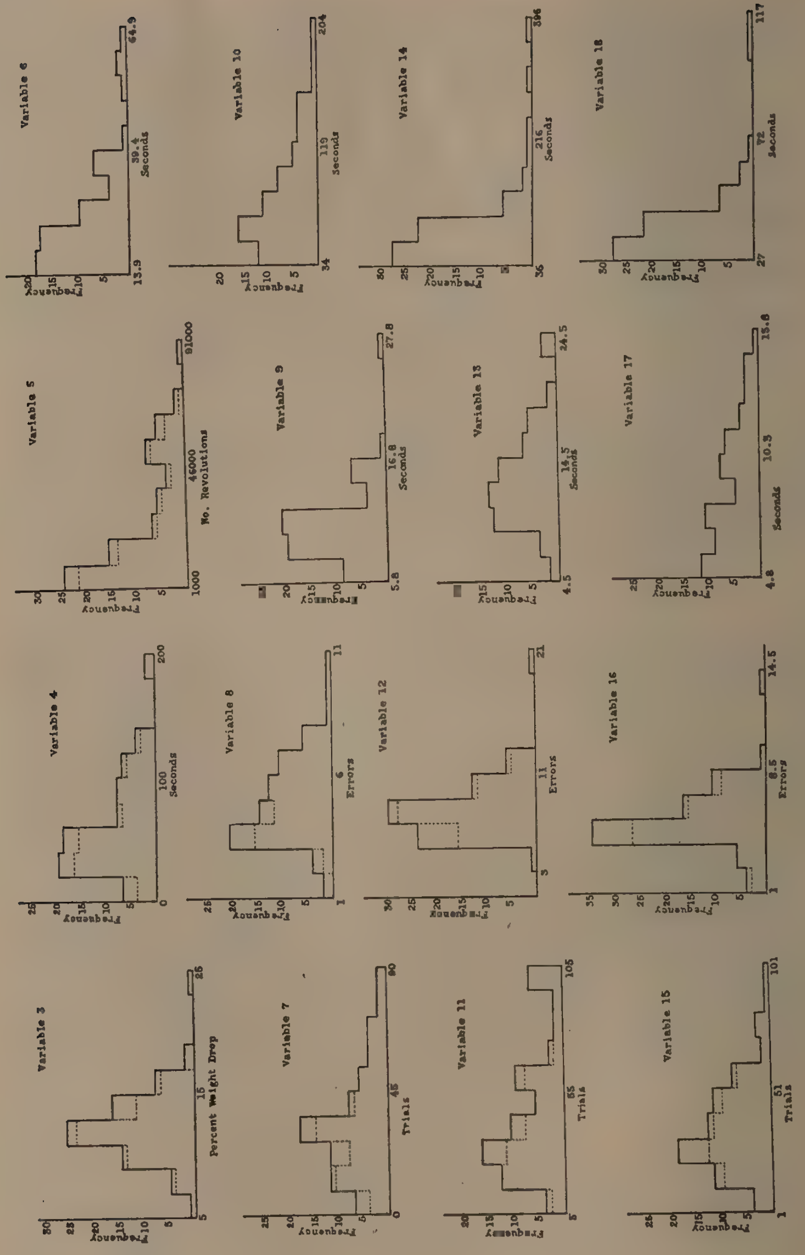

FIG. 11. DISTRIBUTIONS OF RAW SCORES.

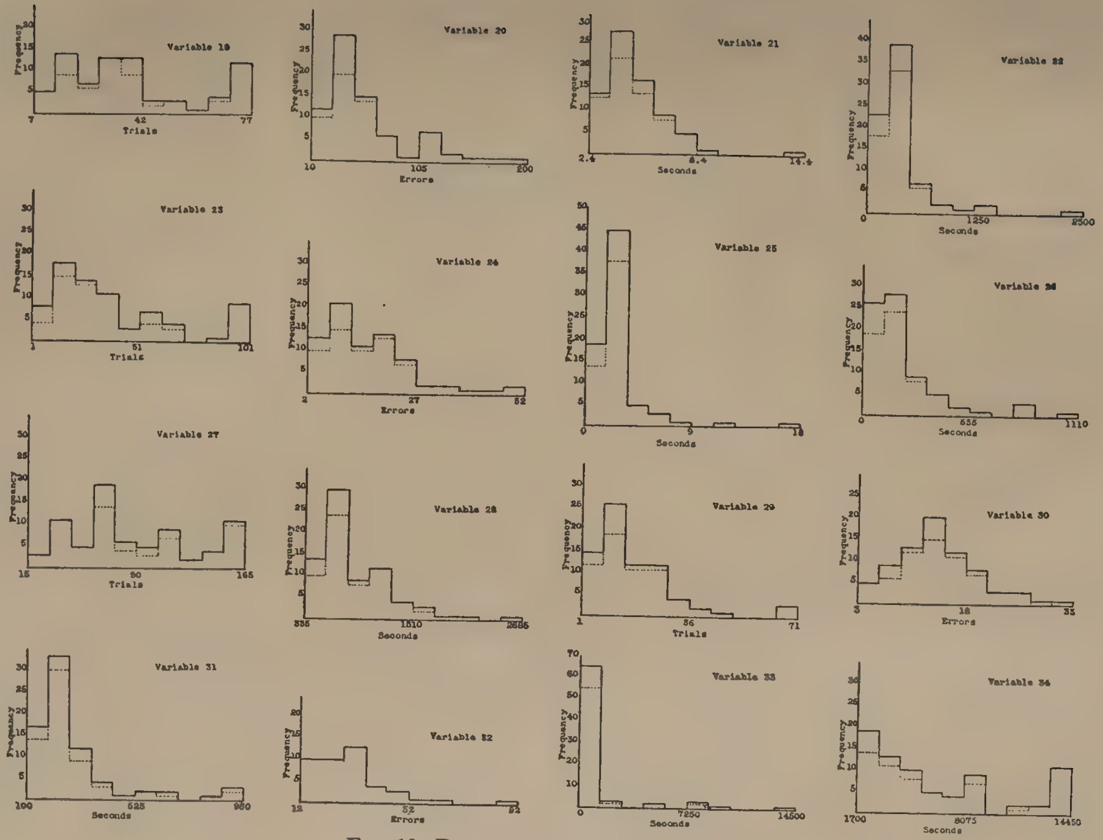

FIG. 12. DISTRIBUTIONS OF RAW SCORES

DUNHAM, H. G.
AUG 22

THE EFFECT OF VOLUNTARY ACTIVITY ON THE KNEE-JERK

SARAH C. DUNLAP

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I. INTRODUCTION

The knee-jerk is at present considered to be a reflex. The history of this question has been presented by Fulton (16), and Fearing (15). The nature of the knee-jerk is fully presented by Wendt (61). The point of action of the stimulus has been shown by Fulton and Pi-Suñer (17) to be sensory fibers within the muscle. It consists of the shortening of an extensor muscle, *quadriceps femoris*, in response to a sudden stretch of the muscle. With all tendon-jerks, it is termed the 'phasic reaction' of the stretch reflex. The other type of stretch reflex, the 'static reaction,' is a response of largely the same neuromuscular mechanism to a slow stretch. Since the blow upon the tendon, serving as the knee-jerk stimulus, is a sudden, quick and brief stretch of the tendon and muscle, practically all the muscle fibers respond simultaneously, except those in refractory phase due to the normal tonic contraction. The lower leg is thus caused to kick forward. This twitch is followed by a secondary contraction, which is a reaction of the deeper lying and slower acting elements of the muscle (7). It is considered to be a "fractional manifestation of the stretch reflex" because of its dependence on tonus (48) and from other evidence (8).

The knee-jerk as described is an unlearned response, judging from the fact that the mechanism reacts with apparently perfect integration upon the first application of the stimulus. The parts of this mechanism have, however, acted previously to the first tendon blow, and presumably *in utero*. The question remains open, therefore, as to whether learning has occurred before the knee-jerk is elicited at birth. It is possible to study any learning which might occur after birth. Tests have not been made from birth onwards to determine whether changes occur, particularly as related to practice in sitting, crawling, standing and walking. Such changes would be influenced, however, by physical development, as for instance the increase in length of the nervous arc necessarily involved.

Another possibility was tried in the present experiment, consisting of the attempt to alter the knee-jerk of the adult through a learning process. There are a number of facts about the knee-jerk which make it plausible to hypothesize that it will be altered by learning, according to Dunlap's view (12). Firstly, it varies in latency or reaction time, degree or height of contraction, and in shape of contraction curve (15). Secondly, the occurrence of other reflex activities is influenced by it, at least in the case of contralateral flexion (45). Thirdly, its variations depend on other concurrent activities of the individual, particularly those requiring cerebral action. It has been shown by Pfaffman and Schlosberg that there is a positive relation between the facilitatory effect of a bell and the ease of conditioning the knee-jerk to it (39). (The conditioning of the jerk is an alteration of the response by changing the stimulus end of it, which is a different kind of alteration from that mentioned above, in which the action is affected.) Whether attention on the knee-jerk facilitates or inhibits it does not appear to be settled. This question is brought up by Fearing (14, 15). It was early suggested by work of Klarfeld (31) that the cerebrum has an inhibiting effect upon the knee-jerk, and this has since been corroborated by other physiologists. Further details will be found below in section III, the historical survey.

II. THE PROBLEM

On the basis of the preceding discussion, the hypothesis is advanced, that repeated concurrent voluntary leg activity, whether it inhibit or facilitate the knee-jerk, will have some effect on the action phase of the response, due to learning. The problem of the present investigation was to determine whether a leg response practiced to a knee-jerk stimulus or its tactual component can become involuntary, and whether the reaction time of such a response may approach with practice that of the knee-jerk; and further, what effect the practice may have on the knee-jerk time.

The specific details of the problem were to determine: whether the knee-jerk can be reversed by practice of a back kick to a knee-jerk stimulus, or whether a kick of the opposite leg can be substituted for it; what may be the time relations of kicks of various types, such as knee-jerks, knee-jerks accompanying the voluntary kicks practiced, the voluntary kicks themselves, and voluntary kicks in response to a touch on the knee or elsewhere; and whether the knee-jerk can be produced, that is, the threshold lowered, by voluntary practice.

The questions arise, whether learning will occur when the subject is not consciously trying to accomplish the response to be learned, when there are no errors to be eliminated, and when there is not definite motivation. The change in the reaction time of the knee-jerk with change in the strength of the blow is also studied.

The method may be said to be that of conditioning provided it is assumed that the voluntary response, if it inhibits the knee-jerk, will eventually replace it as a response to the tendon blow. The conditioning in this case would not be of the knee-jerk, but of the voluntary response. S_1 is whatever sets off R_1, the voluntary response, and may be instructions and intentions, the blow serving merely as a minor part of S_1. S_2 is the blow eliciting the knee-jerk R_2. After repetition of S_1 and S_2 together, S_2 may elicit R_1.

III. HISTORICAL SURVEY

A. Introduction

The experiments of a number of authors tend to demonstrate that voluntary control of reflexes can be gained. Karger has presented data showing that it is possible voluntarily to increase the knee-jerk. This was accomplished by a voluntary contraction of *quadriceps* without allowing the leg to move, begun before the blow (29). This effect seems to be the same as that found earlier by Dodge (9). Peak found that voluntary sets, toward a wink or a finger movement to be made in response to a stimulus to the reflex wink or finger movement, increased the respective reflexes stimulated during the set. The voluntary movement followed the reflex, but anticipations also occurred, since the time interval between the signal for the set and the stimulus to the reflex was constant. The latency of responses recognizable as reflexes was greater with sets than with relaxation, although her results were admittedly confusing (37). Hudgins successfully caused the pupillary reflex to come under voluntary control without the subject's intention or knowledge. He conditioned the reflex to a bell, then the bell itself was rung by the subject by a hand response at the command of the experimenter. Finally the hand response and bell were eliminated, and reinforcements proceeded, the conditioned stimulus being the command. Conditioning was successful even when the subject himself gave the command at the proper signal (26). Steckle and Renshaw did not obtain clear or regular conditioning with a similar procedure, and found that adaptation of the pupillary reflex occurred (50). Gibson and Hudson conditioned the knee-jerk to a facilitating voluntary hand reaction with attention on the reaction, which, however, preceded the knee-jerk. They found transfer of the conditioned response to the other leg to be easy (18). It seems reasonable that voluntary control of the knee-jerk could be obtained in the manner of Hudgins' experiment.

The present experiment does not bear on voluntary control of the knee-jerk, but on the influence of voluntary activity upon it, and in particular on any influence which outlasts the series when a voluntary response was practiced. A similar procedure was employed by Wendt in series taken in order to compare voluntary leg reactions to a blow on the opposite tendon with knee-jerks conditioned to a blow on the opposite tendon (61). He also obtained them to other stimuli. Fourteen of his subjects were engaged in reading while making these voluntary reactions, and two were not. For five of these subjects these series preceded the conditioning series. Most of the subjects reported that the voluntary kicks to a tendon blow became automatic, although in varying degree, and seemed easier to perform than those in response to other stimuli. With a ready-signal they ranged in reaction time from 100 to 180 sigma, and without the ready-signal, from 120 to 200 sigma. Those to a sound cessation were 40 sigma slower, and those to a touch on the tibia, on one subject, were between the other two types. In 9 out of 12 subjects automatic bilateral responses which appeared after conditioning were increased in frequency after voluntary practice.

There was evidence in some subjects of a decrease in the knee-jerks accompanied by the voluntary kicks. This finding is corroborated by the present work.

Wendt succeeded in altering the reflex pattern by conditioning the knee-jerk to a blow on the opposite knee. The final pattern of response to that blow not only often included a conditioned contraction of voluntary reaction time, but a shorter-latency bilateral contraction of which the latency was similar to that of the secondary contraction of the usual knee-jerk curve. The blow to the other leg preceded the unconditioned stimulus by $\frac{1}{5}$ second. The reaction times were measured from kymographic muscle thickening curves, with a 50 d.v. time line. The reaction time of the first response was variable, the medians for the subjects ranging from 128 to 335 sigma, and it occurred generally to either of the two blows. It was more easily formed in subjects where there was a strong facilitation of the second response by the first blow, and was fairly persistent in some cases. It appeared in 10 of 12 subjects.

Cason found that the reaction time of voluntary winks as measured by the Bergstrom chronoscope was decreased by the conditioning process. He used 500 to 3000 reinforcements of a sound and electric shock, the sound being the conditioned stimulus (4).

Wendt quotes from a report of Dodge (11) concerning experiments containing elements similar to his own, as follows:

> An effort was made to produce crossed reflexes by simultaneously stimulating the two knee-jerks. "In spite of hundreds of cases of simultaneous stimulations true crossed reflexes were never elicited. Similarly, simultaneous stimulation of the right and left final common paths to the quadriceps by voluntary effort and by normal reflex stimulation, respectively, also failed to produce a reconfiguration of the mechanism of the knee-jerk". Pp. 68–69. In another experimental series two knee-jerks were elicited from the same leg at an interval of ½ second. In spite of hundreds of repetitions the second stimulus was never anticipated (61, p. 34).

The relation of higher nervous centers to the knee-jerk should indicate how much of an influence could be expected from voluntary action. Golla and Hettwer say that activity of the cerebral cortex is the greatest factor in the variation of the human knee-jerk (19). Walton and Paul make note of a fact often neglected: that the brain can increase as well as inhibit "reflexes." Higher nervous arcs usually, however, produce more subdued and deliberate "reflexes" than lower arcs (58). Reflexes of this type may be represented by the 'tonic' or 'static' secondary contraction of the knee-jerk, which, however, according to Wendt, are probably mediated by or facilitated through the action of nervous centers higher than the lumbar segments of the spinal cord (61).

The absence of the knee-jerk during sleep has often been reported, although the criterion of sleep is doubtful (27, 32). This absence appears to be an effect of the cerebrum through relaxation. The relation between relaxation and tendon "reflexes" is demonstrated by Hoffmann (23), who concludes that tendon "reflex excitability depends directly upon the state of contraction of the muscle." He finds that decrease of the Achilles jerk, caused by voluntary contraction of the antagonists, is due to a relaxation in the muscle. Dodge, on the other hand, finds that voluntary depression of the knee-jerk is not accomplished through any decrease of tonus (9). The question of relaxation appears to be settled in the reports quoted in section C below.

B. The crossed knee-jerk

Some light may be thrown upon the effect of a voluntary forward kick of the opposite leg by a consideration of the sympathy or antagonism of the knee-jerk of the opposite leg. A positive functional relation between the jerks in the two legs is demonstrated in pathological cases where a 'crossed knee-jerk' has been found. This term sometimes is used to include also the more common adduction of the opposite leg. De Watteville (59) found crossed jerks with the muscular latency the same as for the percussed leg. His cases were probably abnormal. Schwartz (44) reports that in cases of cortical epilepsy and of cerebral hemiplegia with increase of tendon reflexes, often the knee-jerk springs from one side to the other when either tendon is percussed. Gotch found crossed extension without the ipsilateral jerk in man, under abnormal conditions (20). He recorded them by means of tambours. He found the latency to be longer than that of the leg stimulated: 110 to 120 sigma as compared to 80 to 90, and the muscle response was less synchronous. Souques and Chauvet (49) found in a hemiplegic whose right knee-jerk was exaggerated, a strong right extension in response to percussion of the left leg, occurring a little later than the left knee-jerk. They state that such results, including crossed adduction, have been noted by Thuë, Féré, P. Marie and Ganault. Wiersma states that the knee-jerk spreads synchronously to the other knee with pyramidal lesions, the percussed knee being inhibited (62). Austregesilo quotes Benedikt as finding crossed extension (61). Wendt (61, p. 10) gives a list of references to crossed reflexes.

There has been some discussion of the mechanism of crossed jerks. Sternberg opposed the idea of bone transmission, in other words direct contralateral stimulation, as the cause for true crossed knee-jerks (51). Gotch found that the latencies of crossed knee-jerks in the rabbit indicated their reflex nature (20). Russell shows that higher centers are not necessary for the occurrence of crossed knee-extension in dogs and rabbits (42). It could be elicited in dogs after section of the corpus callosum when reflex excitability had been increased by induced asphyxia. It still occurred after a transverse section of the spinal cord in the mid-dorsal region. It was then elicitable even to a blow on the tibia, but without any ipsilateral jerk.

Crossed knee-jerks were found by Tuttle in a normal, hyperirritable subject. In this case the knee-jerks were high and easily affected by the environment. A simultaneous kick of the left leg to a blow on the right was obtained 144 times while the subject was engaged in conversation. There was a correlation of .97 between the heights of kick in the two legs, showing that whatever affected the right also affected the left (55). Wendt found that one of his subjects had, before conditioning, a bilateral response following the knee-jerk at a brief interval. This was similar to the response which appeared in other series after conditioning.

The possibility of a normal close relation between the jerks in the two knees is opposed by Sherrington's results. In his extensive work on animal preparations he found a relation between flexion in one leg and extension in the other. He found, however, that in a decerebrate cat the isolated vastocrurei could be made to contract and relax together with equal stimulation of both afferent nerves, provided the conditions of the muscles were similar (47). Minkowski found crossed extension occurring with flexion in the human foetus (34). This has also been observed in spastic paralysis (55). Wendt suggests that the integration between the receptors to stretch in one extensor, and the contralateral extensor, may be on a higher level than that between them and the ipsilateral extensor (61). Supposing the integration with the contralateral flexor to be at the lowest reflex level, the two opposed tendencies in the opposite leg would slightly succeed one another.

Mitchell and Lewis state that tapping the tendon of the opposite leg does not increase the knee-jerk (35). Cornil and Goldenfoun, however, used such a blow as facilitation in conditioning the knee-jerk (61, p. 33). Wendt found that the effect depended on the time relations but that facilitation occurred if the facilitating blow preceded the knee-jerk stimulus by 100 to 300 sigma. With simultaneous blows and at a 500-sigma interval the excitability was lessened. He suggests that facilitation at the longer intervals "involves discharge from higher centers than that at short intervals" (61). A special functional connection between the knee-jerks is not considered, however, necessary to facilitation. In regard to the records he made of voluntary kicks, it may be remarked that if there were no functional connection between the two knee-jerks we should expect the reactions to sound to be the quickest instead of the slowest, or at least that there should not be such a great difference.

C. Certain factors affecting the knee-jerk

The knee-jerk in the present study is viewed as a neuromuscular response following the evidence presented by Hoffmann (25) and others previously mentioned. It may be used in walking; at least walking can include a physiological extension reflex (46, 3) employing the same neuromuscular units as the knee-jerk.

The latency of the knee-jerk was found by Wendt to be 30 to 50 sigma. He used pendulum hammers, electric stimulus marking and a lever and pulley system recording muscular movement on a kymograph. For 18 normal subjects Castex found the minimum muscle thickening latency to range from 29 to 40 sigma (6). Fearing (15) gives the earlier results. Muscle thickening records taken with a tambour containing water are 10 to 50 sigma less than those taken from foot movement (60). The action current latency was found by Nystrom to be 19.4 sigma, and muscle thickening latency taken at the same time to 32.0 sigma on the average for 29 subjects, with a correlation of .783 between them (36).

"Psychical and physical effort" increasing the knee-jerk were found by Golla and Hettwer to decrease the latency measured with the string galvanometer (19). Certain other factors affecting the knee-jerk have special bearing on the present investigation. Wendt gives a summary of a number of facilitating and depressant factors (61, p. 18).

The frequency with which blows are given may affect the reaction time. The refractory phase, according to Dodge, is conspicuously less than that of the eyelid reflex (9). Lee and Kleitman say the extent is influenced by a blow less than 15 seconds previous to the eliciting blow (32). Strughold found the absolute plus relative refractory period for six subjects to be 4 to 5.7 seconds. He used a spring hammer and registration by a string galvanometer, as well as from an air-filled rubber cuff about the muscle. He also found that facilitation affects the refractory phase. It was reduced, on one subject, when a weight acting across a pulley was used to draw his leg back, so that effort was required to keep the leg vertical. The reduction was greater for a heavier weight, and the effects were reversed, when a weight was attached so as to pull the leg forward. The Jendrássik handclasp, and facilitation by raising weights with the hand or other foot, decreased the refractory phase. Strughold measured the refractory period as the first interval between successive blows at which the second jerk was increased above normal (52). This does not mean that the effect of a jerk cannot last more than six seconds. Wendt found that a half-minute interval was sufficient to avoid facilitation effects of one response on the next.

Mechanical reaction time is found to be less with greater intensity of blow in exaggerated knee-jerks, tonus being constant (5), and in normal knee-jerks. Rosenheim found this effect, using a pendulum hammer which made a circuit, and a device on the muscle which broke it, through a marker (41). Weiler used a water-filled tambour (60). Wiersma used 3 blow speeds on 3 subjects with tambour muscle recording (62). Golla and Hettwer found the latency of the action current to decrease with increase in energy of blow. They used progressively weighted pendula, the velocity thus being practically constant (19). This differs from Dodge's finding with the mechanical latency that velocity and not mass produces the effect (9). E. Castex increased regularly the energy of the blow, probably by increasing the velocity, and found that the energy of contraction and the amplitude increased to a maximum, while the reaction time concurrently diminished to a minimum (6). It seems to be true that there is an optimum range for both speed and mass of the blow, dependent on the amount and speed of the muscle stretch which serves as the most efficient stimulus; and that within the range of either, progressive increase can decrease the latency of the response.

H. Piéron has found that the Jendrássik handclasp shortens the knee-jerk latency (40). It is a common observation that it increases the extent since the study of Lombard (33), and similarly it appears reasonable that extent and latency appear in inverse ratio. Varnum confirmed this fact with records from the movement of the foot (56).

The temporal position of a series of records in an experimental sitting usually affects their reaction times. Dodge made extensive tests on one subject in four-hour periods with six knee-jerk stimuli in each hour. The average reaction time for all the days increased during the four hours from 30 sigma in the first to 40 sigma in the fourth hour. Recording was by means of muscle thickening curves photographically registered. The extent showed no systematic changes (11). Similar effects were noted by Rosenheim, who found that the reaction time was increased as more blows were given (41), and by Travis and Young. They averaged the first and last 20 string galvanometer records for each of 5 sittings of 40 records each and found an increase of slightly over .1 sigma of the second 20 over the first 20 (54).

The cause of this decrease may be found in work on knee-jerk extent. Jacobson found the knee-jerk to disappear with supine relaxation (27). With Carlson he found that the knee-jerk decreased parallel to the degree of relaxation (28). In 21 subjects, who were given 27 periods each of 40 to 70 minutes, the knee-jerk extent decreased on the average during a period. Blows were given with a spring hammer, usually at 30-second intervals. Nothing was said to these subjects about relaxation (27).

It was found by L. T. Brown that physical exercise may decrease the knee-jerk by causing excessive fatigue. (2). This effect was limited to non-athletes, and as subjects in the present study devoted little or no time to athletics, it may be a factor contributing to decrease of the knee-jerk. Hoffman found no effect due to normal fatigue caused by voluntary activity (23).

Adaptation due to repetition of the knee-jerk blow does not appear to occur. A report of an experiment considering this question is now in preparation. Hoffman found no adaptation of the Achilles jerk to occur when induction stimuli were applied at 45/second (24). In the study of Dodge (11) last mentioned above, 1032 stimuli were given over a period of 18 months. The reaction times showed "a clear shift of central tendency" towards greater values in frequency distributions of the last few days' records as compared to the first, and the extent was slightly diminished. Lee and Kleitman found that the knee-jerk disappeared in all tests after several months of work on one subject (32). Since it could be obtained with simple facilitation, such as conversation, its disappearance can be explained as due to practice in relaxation, without recourse to adaptation as an explanation. Relaxation also serves to explain the results of Dodge.

IV. SUBJECTS

The subjects of experimentation were 14 in number. The women are designated as follows: Cf, Df, Ef, 1f, 3f, 5f, 6f, and 7f. The men are: Am, Bm, Fm, Gm, 2m, and 4m.

All the subjects except 1f, 6f and 7f were advanced students in psychology. 7f was a graduate student in zoology, and 1f and 6f were college students. 6f was the only one who had never had a course in psychology.

V. FIRST EXPERIMENT

The first experiment was solely an attempt to alter the involuntary response to a knee-jerk stimulus by practice of a different, voluntary leg response to that stimulus.

The subject was seated, with his lower legs hanging free, on a chair cushion curved up at the lateral edges to prevent lateral movement. Shoes and hose were not removed, but the trouser of the leg percussed was rolled to above the knee. A length of flexible rubber rod of half-inch diameter was fastened across the patellar tendon, being crossed behind the knee and clamped above it, as illustrated in figure 1. This rubber was struck by a heavy pendulum hammer just past its center of swing. The practice of placing an object across the tendon, so that variations in the position of the leg will not affect the point of application of the blow appears to have been initiated by Hoffman (22). The pendulum was dropped from an angle constant for each subject and large enough to elicit a visible knee-jerk before training. It was operated by hand, and the angle from the vertical was judged by a scale beside it. For subjects Am and Df, all stimuli but the last 1700 were given with a shoemaker's hammer.

The pendulum was 60.76 cm. in length, the bob weighed 857.0 grams and its period was 1.57 seconds. For subjects Am, Df and Gm, and for Fm at first and Bm for a brief time, it was dropped from about 26°15′. The velocity at the lowest point in the swing was 93 cm. per second, calculated by the formula

$$v = \frac{2\pi l \sin \phi}{T}$$

where v = velocity, l = length of the pendulum, T = its period, and ϕ = the angle from which it was dropped. This measure of course is not accurate, since it was not a simple pendulum. For subjects Bm, Cf, Ef and Fm the angle was 22°30′ and the velocity 107 cm. per second.

The pendulum support was adjustable in all directions. It could be turned so that the direction of the blow was in line with the upper leg of the subject. Curtains were hung so that he could not see the pendulum, his knees, or the experimenter;

except in the early work with subjects Am and Df, who kept their eyes closed instead. A tilted writing board could be set up to rest books or papers on, with lighting from the proper direction.

Subjects Am, Bm and Cf practiced a back kick of the left leg in response to a blow on the left leg, and subjects Df, Ef, Fm and Gm practiced a contralateral forward kick, the left leg responding to a blow on the right. The instructions were written and presented each day, except for Am and Df. The voluntary kick was to be made immediately after the blow, as quickly as possible. With the contralateral kick, attention was to be kept on the left leg.

It is difficult to tell how much the subjects guessed concerning the experiment. The set-up was such that they must have known they were practicing the response, since no records were taken. They tried not to be too interested, Cf apparently being most successful. Am and Df knew the precise object of the practice, as well as what was being observed.

Subjects Am and Df were given training series at intervals of from one to eight days, with systematized testing only on the last three days. The other five subjects were given training usually three times a week, with certain lapses which will be indicated. A training series was given on each experimental day and consisted of 100 blows given at irregular intervals during slightly less than 15 minutes. At first the blows were given in groups of five with a ready-signal, or with a ready-signal for each blow. On later days there was a ready-signal only at the beginning of the series and after any interruption.

To test the effect of the training, at first an unexpected blow was given. On days when this was done, the subjects Bm, Cf, Ef, Fm and Gm were instructed to pay no attention to blows during "adjustment of the apparatus." They may have thought they were expected to kick voluntarily despite the absence of a signal: if in doubt, one would expect them to do so. Am and Df, however, knew that automatic kicks were allowed but not voluntary ones. They found the distinction difficult. Am observed that he could not tell how involuntary a response was when he was not attending to it. The reports as to whether a kick was voluntary or not may be taken as honest but not necessarily conclusive. These reports were in most cases in response to questioning, and the questions were not emphasized, other questions being also asked.

All the subjects showed some responses modified from a knee-jerk to these test blows. These will not be reported in detail. To control attention and expectation of response, testing was substituted, which was carried out while the subject worked at the Number Work Test—Johns Hopkins Series, which requires continuous addition and subtraction. The subject's instructions were to work aloud and continuously, paying no attention to what the experimenter did to his legs. The subjects could not help noticing when their legs were hit, but usually succeeded in not observing them expectantly. The experimenter did not give a blow unless the subject could be heard to be working evenly. Ten, or usually five tests were taken in this way before and after each training series. Sometimes, also, an unexpected blow was given at some other time, the results in such cases being no different from those obtained during number work. The object of this method of testing was to keep the attention of the subject distracted so that automatic and reflex kicks would not be inhibited.

The visible knee-jerks occurring with the voluntary contralateral kicks or before the back kicks were counted for each training series. The instructions to attend to the left leg when the right was being percussed were intended to combat any effect of attention on the right knee-jerk in the case of the contralateral kick. The number of visible knee-jerks varied greatly in each subject and between subjects. Knee-jerks occurred before back kicks although the subject kicked back as quickly as possible. They usually became less frequent toward the end of a series; and in four of six subjects they decreased generally toward the end of training. The exceptions were Fm and Gm, who received the least practice and who showed no consistent changes.

The responses to test blows included large numbers of knee-jerks such as appeared before any training was begun. If they were small, they were noted as modifications of the response. Other modifications of the knee-jerk response were of these types: a kick like the one practiced (a back kick or a contralateral kick), such a kick preceded by a knee-jerk, or the absence of any visible response. Reaction times and extents were not measured, only visible leg motion being recorded. Judgment of a knee-jerk as "small" was beyond doubt subjective.

The records included in table I are those taken on days when the number work test was used as a distraction. They were all taken during the winter. For Am and Df they include also series without distraction, and the blow speeds were varied.

TABLE I

SUBJECT	TOTAL PRACTICE BLOWS	EXPERIMENTAL DAY TESTS BEGUN	TOTAL TEST BLOWS	NUMBER APPEARING IN TESTS OF:			
				BK	KJ and BK	No response	Small KJ
Am	3100	29	151	1	16	23	14
Bm	2800	7	258	2	1	2	1
Cf	3100	6	313	3	15	6	7
				LFK	RKJ and LFK	No response	Small KJ
Df	3100	29	129	10	51	9	
Ef	2900	7	270	1	25	39	101
Fm	900	6	71	0	8	0	0
Gm	1700	1	185	0	3	42	59

The abbreviations used in the tables are as follows: BK for back kick, KJ for knee-jerk, and FK for forward kick.

No extended testing previous to all practice was considered necessary for comparison with the tests reported here. The back and contralateral kicks reported were certainly due to the practice. The percentage of modified responses of each type for each subject was greater in all tests following the training series with 7 exceptions, none of which fell in the BK or LFK category. The results from Fm are questioned, as he gave the experimenter the impression that he might not be following instructions. The best results in terms of modifications of the response are evident with Am and Df, and these were obtained in test series more extended than for other subjects, in which a relatively greater disappearance of modifications would ordinarily be expected. These tests were not made until late in the experiment, although modified responses appeared earlier in other tests. The more frequent testing of the other subjects may have had effects opposed to those of the training.

For subject Am, modified responses to test blows appeared from day 6 on, and were usually knee-jerks followed by back kicks. Eight times during the experiment he reported that back kicks made to test blows were involuntary. On a different occasion he was intentionally expecting a knee-jerk to follow a blow, but a back kick was elicited. Several test blows while he was 'relaxed' and more or less expecting knee-jerks, elicited knee-jerks and a few small back kicks. He noted that the back kicks became automatic; certain ones, more than 18 in number, he remarked as definitely involuntary. Of these, one was the first response of a series, when he was confused and kicked forward after it. Once there was an involuntary back kick of both legs. Another time he forgot what he was to do, but the kicks occurred as usual.

He tried, further, definite distractions of attention during practice. He first observed that the kicks seemed more violent when he was attending to something else. Several days later he observed that when his attention was not on his legs the kicks were not so forceful, although they occurred regularly. Once, however, he failed to kick at all when he was not attending, but at other times this was not the case. Twice when he put his attention on his right leg it also kicked back. When he performed a hand movement in response to the blow, on the twenty-eighth day of practice, he was unaware of the back kicks, although they did not fail to occur. On the thirtieth day he reported that he was not conscious of the back kicks. On the fourth day the left leg began to feel different from the right. This difference later became definitely noticeable to him when walking. Near the end of practice the leg sometimes gave way under him, and he found from time to time that he was walking flat-footed on the left leg.

Twenty-three days after the last training series an involuntary back kick could be elicited.

The first modified response of subject Bm appeared on the ninth day. There was then a lapse in training of 18 days, after which for some time only knee-jerks were obtained in tests. It was noticed in practice that the right leg sometimes kicked back, involuntarily as determined by questioning. Later it tended to move forward, and these kicks were very brisk on a day when he had just run upstairs. He was questioned about a back kick occurring in the last test series, and did not know whether he meant to kick or not.

Subject Cf showed two modified responses, both back kicks, to test blows on the first and seventh days, which she reported were automatic. There was a lapse in practice of 16 days after the eighth day. From the tenth day of practice, the right leg usually also kicked back slightly, more often in the first part of a series. Eight months after the last training series, records were taken with the Johns Hopkins chronoscope, using a pendulum of which the bob weighed, with a contact device on it that was used in the second experiment, 1052.3 grams. For a fall of 21°, the velocity is 87 cm. per second.; for 6°, 25 cm. per second. The break in the front magnet circuit of the chronoscope was made by a momentum device on the pendulum, to be described with the second experiment. The leg kicked against a rubber bulb close to the shin. This was connected by a tube to a pneumatic key, which broke the circuit through the back magnet. The reaction times are obviously lengthened and are of value only for rough comparisons. The knee-jerk records were taken with the number work test as distraction.

A range of 120 to 147 sigma was obtained for 10 left knee-jerks to a blow from 21°. Nine of them were followed by back kicks. Following this series, 16 left forward voluntary kicks were recorded to a left blow of 6°, and ranged from 205 to 253 sigma. This blow was too slow to elicit a knee-jerk. Ten right knee-jerks were recorded to a 21° blow, and ranged from 117 to 160 sigma. The average was insignificantly less than that for the left knee-jerk. Ten right voluntary forward kicks to a right blow of 6° followed, ranging from 230 to 293 sigma. She said that she found the left forward kick difficult, since there was a tendency to kick back. These records indicate that the knee-jerk was unaffected, but that the habit of kicking back was added. Since the back kick was not timed in practice series so as to eliminate the knee-jerk, it may not have had any inhibiting effect.

About five months later the following records were taken, with a 20-second interval between blows as a rule, and before the series of voluntary back kicks a rest period during which the subject walked down a long hall and back. The blow used was from 18° with a light pendulum bob, for which the velocity is given on page 23. The method and recording were those of the second experiment. During the knee-jerk tests she was reading an absorbing story. The order of the series was: 30 left and 30 right knee-jerks in the order LRRL (15L, 30R, 15L), 41 left back kicks with the preceding knee-jerks recorded, and a second series of knee-jerks in the order LRRL.

A few records are omitted because of anticipations and experimenter's errors. The averages in sigma were: RKJ 77.7, LKJ 76.7, LKJ preceding LBK 100.7, RKJ 81.6, and LKJ 85.1, the medians being slightly less. The LKJ with LBK had a mean variation of 13.4; the others ranged from 2.6 to 7.5, the differences due apparently to the succession of the series. During back kicks, the bar against which the leg rested (described with the second experiment) had to be left off. The different position of the leg when no bar was holding it forward could not cause such an increase as appears in the LKJ with LBK. The first four knee-jerk records following practice were taken without the bar behind the leg so that involuntary back kicks would be possible, but they did not occur. The records were consistent with the rest of the series, and are therefore included in the average.

Since the immediate effect of the practice did not last definitely for more than ten records, the first ten R and L records were averaged separately in the series preceding and following the BK series. The first ten RKJ averaged 78.3, and the LKJ 74.5; in the second KJ series the average of the first ten RKJ was 79.2; LKJ, 89.9. The mean variations were 2.3, 2.5, 4.6 and 7.3 respectively. The LKJ records are lower in the first series than the RKJ because they were taken first, and the greatest increase seems always to come in the first few minutes of a sitting. In the second series, although taken first, the LKJ records are higher and the range is extended upward, indicating a slowing up due to the back kick practice. This is evidence of a transitory effect of the voluntary practice upon the knee-jerk. It is further evident that the intention of kicking back definitely lengthened the reaction time in the LKJ series with back kicks.

Unexpected tests on subject Df frequently elicited modified responses, some of which were contralateral kicks alone. Most of these seemed involuntary to the subject. Twenty-three days after the end of practice, a strong left kick accompanied right voluntary back kicks made to blows on the right leg. These occurred although she concentrated her attention as much as possible on the right leg. The blows used were of knee-jerk speed and less. Ten months later in a sitting of about an hour and a half, records were taken with the pneumatic system previously mentioned operating the chronoscope. The subject was engaged in composing poetry and managed to keep her attention fairly evenly occupied, although, as happened in all cases, she knew what was going on most of the time. The blow was given from 24° with the pendulum bob and contact device weighing 1052.3 grams. The velocity was 99 cm. per second.

Forty-two left forward kicks reported involuntary were obtained in response to 45 right blows, and ranged from 180 to 315 sigma; then 50 RKJ's to 59 blows, ranging from 98 to 200 sigma. Left kicks occurred with the knee-jerks recorded, and once when no knee-jerk was elicited there was a violent left kick. The left kicks showed no approach to reflex reaction time. The apparatus probably exhibited a greater lag in the case of small knee-jerks, causing times up to 200 sigma.

The first and only left kick without a knee-jerk in tests on subject Ef was elicited on day 9; other modifications appeared earlier. There was then a lapse in practice of 18 days, followed by a decrease in the number of modified responses to tests. They increased again, but toward the end of practice the kick of both legs became less frequent. She stated on the first day that she could not kick the left leg without deadening the right, but her knee-jerks were at least as frequent as those of any other subject. One test kick of both legs she reported to be involuntary.

Forty-two days after the last day of practice, no modified responses could be obtained either before or after a training series, which was given with a mechanical system in use for operating the chronoscope. Seven months later, records were taken with the pneumatic system in use, and the pendulum bob weighing 1052.3 grams, as for subject Cf. No recordable knee-jerks nor involuntary left kicks could be obtained either before or after voluntary practice, probably because of the insensitivity of the apparatus, although they appeared during the voluntary practice series. A range of 178 to 220 sigma was obtained for a voluntary LFK to 22 right blows, some from 21° and some from 6°; and of 200 to 377 for a voluntary RFK to 20 right blows from 21° and 6°. Only the usual effect of reaction time practice is evident in the difference between the series.

Knee-jerks were obtained with the voluntary left kicks to a right blow. With the first, second and tenth voluntary right kicks, left kicks occurred. She was told that she had no recordable knee-jerk. Subsequently, two very small left kicks were obtained with three blows to the right leg, while she was attempting to 'relax' or allow knee-jerks to occur. The left kick may be said to have become in some degree habitual.

Subject Fm received much less practice than those discussed above. Five of the kicks of both legs, elicited in tests, occurred on the ninth day, just after a lapse of 73 days. Kicks elicited by unexpected blows in the first few days had, however, been suspected of being voluntary or at least expected, since Fm appeared to be interested in what the problem might be. He might also have misunderstood the instructions and thought that he should kick although no ready-signal had been given.

The kicks of both legs of subject Gm elicited by test blows appeared only toward the end of training, on the thirteenth and fourteenth days. On the sixteenth day, to an accidental blow following the first number work, he kicked the left leg. He said he did not know why he kicked, and the kick may have been involuntary.

There appeared to be a greater and more lasting automatization of the practiced response in the subjects, Am and Df, who knew what the hypothesis was. They were also the ones who were not tested much until after considerable practice, and whose practice suffered no lapses. A similar frequency of modified responses appears in Fm, who also may not have been impartial.

Further work was felt to be necessary, employing reaction times for comparing practiced kicks and knee-jerks. It was desirable to find whether the knee-jerk reaction time changed with practice of another response to the blow, and to try practice when the subject could not guess what was the main purpose of the measurements.

VI. SECOND EXPERIMENT

A. Apparatus and methods

The method employed for measuring the reaction times of leg movements was such that frequent practice was possible with the preservation of a record of every response. The Johns Hopkins chronoscope was used, the motor being operated on rectified synchronized 60-cycle alternating current. The unit of measure was $1\frac{2}{3}$ sigma (millisecond). The chronoscope hand was set in motion by a momentum current-break on the stimulating pendulum, and was stopped by a break caused by the foot movement.

The set-up was intended to avoid the difficulties encountered in other devices for measuring knee-jerk reaction times. Dodge finds direct records from the muscle to be the best type, since inertia is reduced to a minimum. Pneumatic recorders are too variable. The difficulty with mechanical muscle-contraction recording, however, is that the contraction curves vary according to the part of the muscle from which the records are taken (9). Contraction curves, moreover, are difficult to measure, because the beginning of the contraction must be determined arbitrarily for different shapes of curves. The break device used in the present experiment is possibly also subject to this criticism, in that the part of the contraction when the contact broke was unknown, and may have varied.

Dodge was the first experimenter to use a momentum current break device on the pendulum and a current break from the foot, but he recorded the breaks on a kymograph and the latencies of the markers are unknown. As he reported none of the measures obtained by this method, comparisons cannot be drawn with the measures to be reported below. With graphic registration, few records can be taken in a sitting. He did not obtain more than 22 records on any one point (9).

The present records had to be taken from the foot because no accurate break device could be found for use on the muscle. One highly sensitive device was contrived, but proved inadequate because the mechanical effect of the tendon blow caused a break.

A pendulum, as Dodge mentions (9), is the best hammer because the blow strength is measurable. The one used for stimulation in the present work was operated as in the first experiment, with a more accurate scale graduated in units of 2°. The error in dropping it reached 1° rarely. Fastened across the tendon was a flexible rubber rod of slightly smaller diameter than before, $\frac{13}{32}$ inches. A lighter pendulum bob was used except where otherwise noted. With the contact device mounted, it weighed 430.2 grams. The velocity at the lowest point of swing was calculated in cm. per second, by the formula previously given, for each angle of fall used, as follows: for 4°, 17; 6°, 25; 8°, 34; 10°, 42; 12°, 51; 14°, 59; 16°, 67; 18°, 75; 20°, 83; 22°, 91; 24°, 99; 26°, 107; 28°, 114; 30°, 122; 32°, 129; 37°30′, 148. The use of the heavier bob increased the momentum and energy of the blow, but not the velocity, which with a pendulum of this description seems to be the major factor.

The contact break device on the pendulum was a vertical lever pivoted at the top. It was held against a contact by a coil spring. When the pendulum hit the knee, momentum caused the contact to break temporarily. In the circuit that started the chronoscope was the break device and a balanced relay which remade the circuit, shunting out the break device. A silk cord, attached to the back of a tape around the narrowest part of the subject's ankle, was connected to another break device, in the circuit that stopped the chronoscope. This was a lever pivoted

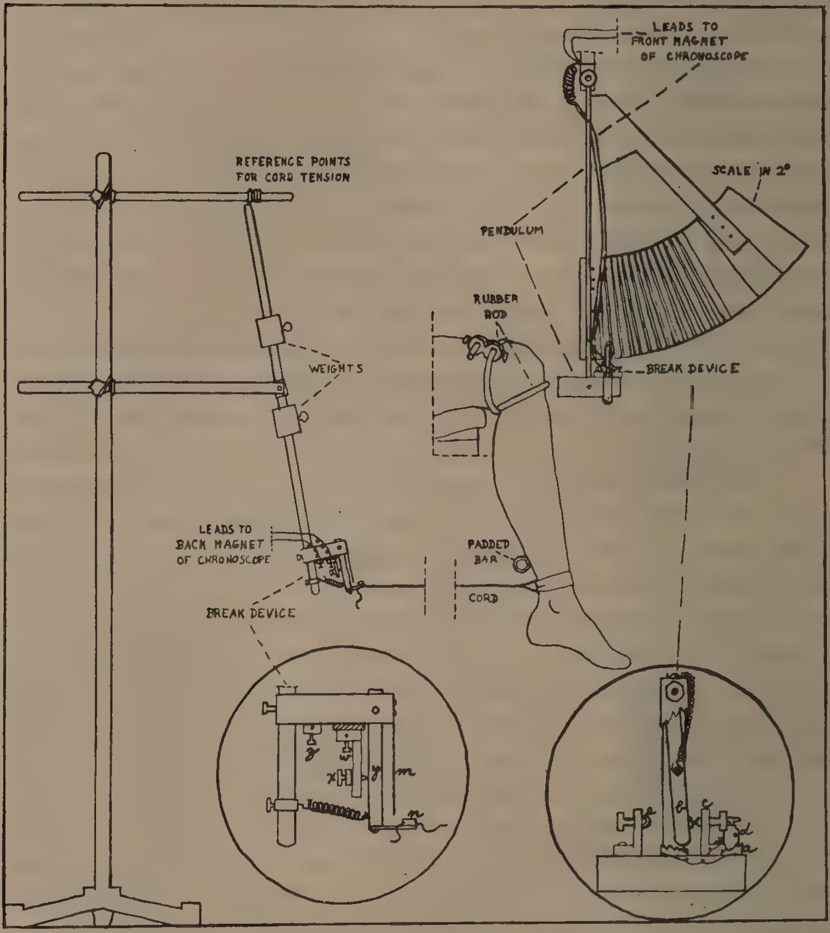

FIG. 1. ESSENTIAL PARTS OF THE APPARATUS

Left inset 0.2 actual size, right inset 0.4 actual size

a, binding post (in part omitted); *b*, contact lever (near upright in part omitted); *c*, contact; *d*, binding post; *e*, rubber stop for lever; *m*, steel spring stop for lever; *n*, set screw for cord adjustment; *w*, binding post; *x*, contact; *y*, contact lever; *z*, binding post. Circuits: *abcd*, *wxyz*.

at the base of a compound pendulum bearing weights above and below its fulcrum. The lever was held back against a contact by a coil spring. The weights, the spring and the tension of the cord were adjusted so that a quick forward movement of the foot broke the contact but a slow one did not, the whole pendulum moving with the foot in the latter case. For each subject the string tension was adjusted so that the slight mechanical movement of the leg due to the tendon blow just failed to break the contact.

The current through the chronoscope magnets was made equal by a shunt resistance across each of them. The balanced relay had always one magnet in each of the chronoscope magnet circuits.

The chronoscope and balanced relay were enclosed in a felt-lined wooden box with a window in the front. This diminished the sound of the clicks considerably. For a masking noise a relay was used, connected to a contact on a vibrating reed. This gave a continual clicking sound which was present during all the sittings for which records are reported. The experimenter could still hear the chronoscope clicks, but every subject claimed that he could not, when at the end of the experiment he was asked how the clicks varied. The faster blows were audible, but this could not be avoided.

The subject was seated on a table on a chair cushion raised at the forward end, and raised more at the sides to prevent lateral movement of the legs. Curtains were drawn before him and there was a board on which to rest books or papers. The final set-up included a wooden back rest adjustable as to position and angle, and there was a padded bar behind the subject's legs just above the ankle. The bar was horizontal, and was moved forward so that the legs rested against it, thus keeping them in a constant position and preventing the jar of the blow from moving the ankle. The exact position of the bar was not important, as it could not affect the position of the leg appreciably. It has been said that passive extension of the knee does not affect the mechanical knee-jerk reaction time (60). The subjects' shoes were removed during all the series for which records are reported, except for subjects 3f and 6f, who never wore heavy shoes. Their feet swelled uncomfortably if shoes were left off. Hose were removed or rolled down or not, according to convenience. The trouser leg was rolled up when the leg was to be percussed. When the subject was kicking voluntarily, a rubber bulb was placed about a half an inch before the shin to break the force of the kicks.

Sittings rarely lasted over an hour. During this time the subjects' legs in rare instances became a little numb, but they never considered the discomfort sufficient for complaint. The fact may bear on the decrease apparent in the jerks, along with other factors previously discussed, since, according to Tuttle, when the leg actually "goes to sleep" the jerk may be depressed (55).

The set-up was in the corner of a large room in which, frequently, another person familiar to all subjects was quietly working. Other people sometimes entered, but almost never could see or be seen by the subject, and there was sometimes audible conversation. The subjects were almost never disturbed by such occurrences, which were probably minimized by the clicking relay. When records were being taken for comparison of the right and left knee-jerks, special care was taken to keep conditions constant and the room quiet.

The subjects could not fail to know from which foot records were being taken and to which leg the blow would be given. Since it took a few minutes to adjust the pendulum, all blows were given to the right leg except when left knee-jerk records were necessary. The right leg may have become more accustomed to the attachment of the rubber and to the blows. The effect of this procedure on the knee-jerk reaction times is not ascertainable.

No records of the monthly cycles of the women subjects were kept, since their effect on the reaction times measured is not known. It is probably variable, judging from work on extent (30).

Subject 7f was tested for the effect of the time interval between knee-jerk blows, in sittings of an hour and a quarter on two successive afternoons. She had previously had one sitting in the set-up and was not used in any other part of the experiment. She was occupied in reading short stories. In the first sitting 127 blows were given from 26°. Sixty records were obtained with a stimulus interval of 40 seconds, followed by 10 at 15 seconds. In the second sitting 252 blows were given from 26° and the following series of records obtained: 60 at 15 seconds, 11 at 40 seconds, 33 at 20 seconds, 11 at 15 seconds, 17 at 25 seconds, and 14 at 15 seconds. No significant differences were visible in the distributions of reaction times. It was further noted that no change in time interval, and no lapse of a minute or more during which adjustments were made, affected the reaction times. The first two records following 17 such changes were found to fall evenly through the distributions. Where definitely wide kicks were seen, the reaction times fell at the short end of the distributions. Of these there were 13.

Subject 3f was tested on January 22 with 109 blows from 18°. Twenty-two records were obtained at a 15-second interval, 33 at 20 seconds, and 10 more at 15 seconds, showing no difference in the effects of the two intervals.

The frequency of knee-jerks decreased in both subjects toward the end of a period. The successive distributions were shifted toward slightly longer reaction times throughout a period. These changes seemed to occur in all the sittings to be reported. A longer series, due to this fact, showed a distribution of reaction times extended at the upper values. The average, compared to that of a briefer series, was increased more than was the median. In a series of order LRRL, such as 15 L, 30 R and 15 L records, the left knee-jerks would be expected to show wider variation than the right, assuming no original difference between the jerks.

It was determined to employ an interval of approximately 20 seconds between blows in all work. This interval was intentionally not kept accurate, but did not vary more than 3 seconds either way except when a temporary stop was necessary. After the last sitting, each subject was asked how regular this had seemed. It seemed fairly regular to 1f and 5f. 3f, 6f and Cf said it was not; 2m supposed it took longer after he gave a knee-jerk than when he did not. None of them seemed to have given it any consideration.

The period between the ready-signal and the blow for voluntary kicks was varied from 1 to 2½ seconds, the experimenter gauging the time by the clicks of the masking relay. No subject had any idea that this was done, although they all knew, except 3f, that the preparatory period was irregular.

The effect of the initial velocity of the kick on the break of the pendulum that stopped the chronoscope has not been determined, nor has the effect of the tension of the cord. The most sensitive setting was used on 1f, 4m, 5f and Cf. The next was used on 2m, 3f and 6f, and the least sensitive on 4m after the 7th sitting. The distributions of knee-jerk records obtained with 4m show no difference for the two settings.

B. Experiments and results

Records were taken, on subjects 3f, 4m, 5f and 6f, of the responses listed below, for the purpose of comparison. Abbreviations used are given in parentheses. VK is the same as the FK of the first experiment. The series of records were made in the order given, in any one sitting, and this order was reversed on successive experimental days so that it did not affect the averages.

1. Knee-jerk of the right leg (RKJ).
2. Voluntary forward kick of the right leg (RVK to blow R) following a very slow (i.e., small pendulum-angle) tendon blow. This blow was just quick enough to be easily perceptible.
3. Knee-jerk of the right leg while the subject was making a voluntary forward kick of the left leg in response to a blow on the right leg (RKJ with LVK).
4. Voluntary forward kick of the left leg to a blow, on the right leg (LVK to blow R), of knee-jerk speed. This is just like response 3 except that the records are taken from the other leg. The blow used was definitely above the threshold speed for knee-jerks.
5. Voluntary forward kick of the left leg to a very slow blow on the right leg, of the same speed as was used for response *2*.

The determinations of the threshold of any subject were never very accurate when only a few blows were used. In cases where they were accurate, the data are considered under *Blow speed and reaction time.*

Further practice was given subjects 3f, 5f and 6f in responses *4* and *5*, with records also of *3*. Practice was given subjects 1f, 2m and 4m in response *2* with faster blows. Further knee-jerk tests were made. Table II is shown for reference.

Instructions were formulated and typed, and the appropriate set was read by the reactor immediately before each series, as follows:

1. You will be given a series of blows on your right knee. Before each blow I shall say "ready," and immediately after each blow you are to kick the right leg forward as quickly as possible. Keep in mind what you are to do: at each blow on the right leg that is preceded by a "ready" signal, kick the right leg forward as quickly as possible.

TABLE II

Total practice

SUBJECT	LVK TO KJ BLOW R	LVK TO DEFINITELY SUB-KJ TOUCH R	RVK TO DEFINITELY SUB-KJ TOUCH R	RVK TO MODERATE BLOW R
1f				1511
2m				1669
3f	563	219	1291	
4m	339	121	120	748
5f	1657	681	196	
6f	2043	848	245	

2. You will be given a series of blows on the right knee. Before each blow I shall say "ready," and immediately after each blow you are to kick the left leg forward as quickly as possible, keeping your attention on the left leg. Keep in mind what you are to do: at each blow on the right leg that is preceded by a "ready" signal, kick the left leg forward as quickly as possible.

3. You will be given some number work to do. Work continuously and aloud, paying attention strictly to the number work. Start when I tell you and continue until I tell you to stop.

The number work test, carefully explained to the subjects, was used as a distraction except when extended knee-jerk records were taken; in the latter case the reactor being instructed to keep his attention on reading matter and to pay no attention to his legs.

A few unexpected test blows were employed, but as the subjects expressed no surprise that kicks occurred, they must be classed as voluntary.

The subjects were asked after the last sitting what they knew about the purpose of the experiment. They mentioned the fact, which the experimenter had originally told them, that comparisons of voluntary and reflex reaction times were being made. Subject 4m had heard the title of the problem before the last sitting, but could not say what he thought the results should be. Subject 2m did know that an attempt was being made to obtain a better knee-jerk but considered this incidental.

Details of the work will be discussed separately for each subject, with tabulation of the records. Individual records were taken to the nearest sigma. Records for certain responses are not included, as follows: (*a*) cases when there was no response large enough to stop the chronoscope; (*b*) anticipations, wherever the subject noticed them, or where the records were well below the range of other reaction times; (*c*) cases where the experimenter or subject made an error, or if there was some definite disturbance of the subject. Records of both classes (*b*) and (*c*) are probably still included in the records presented, as it was desired to avoid the selection of results in so far as possible.

Subject 1f. 1f reported at the same hour every afternoon. She showed no knee-jerks without facilitation, but kicks slower than knee-jerks occurred when she was tested, while reading short stories. They will be referred to as 'imitation knee-jerks,' and occurred even after she was informed that her knee-jerk was all right, that it was natural for it to disappear while she was resting. As she was a foreigner who had not been speaking English many years, there is a possibility that she did not understand very well. On the last day, she was asked why it was bad not to have a knee-jerk, and replied that it was indicative of one's mental state.

On March 5 and 6, to 92 right blows of from 16° to 32°, 32 records were obtained ranging from 102 to 1060 sigma, with an average of 379.3, and mean variation of 120.2. The Jendrássik handclasp was tried with one blow each at 20° and 18° and 2 blows each at 16° and 14°. Two records were obtained, with 16° and 18° blows, of 92 and 93 sigma. Only one imitation jerk was recorded in this series.

She was given practice in a RVK to a R blow definitely too slow at the beginning to elicit any knee-jerk. The data are in table III. Anticipations were difficult to count and are probably underestimated. They averaged 2 per cent of all the kicks, while other subjects' were less than 1 per cent. They decreased in frequency for all subjects with practice. Four of 6 on March 6 occurred soon after she learned that the apparatus measured time. No other subject was so naive. A total of 25 records of less than 95 sigma are included on March 7 and March 12 through 23, and 59 records between 95 and 113 sigma. It is difficult to determine whether these and higher records were anticipations or knee-jerks. Many of them definitely were not anticipations, as far as the eye could tell, as kicks succeeded them. The preparation to kick should have been sufficient facilitation for knee-jerks to occur, as from March 9 the experimenter noticed that she almost always contracted her muscles after the ready-signal so that the leg moved visibly. When questioned on March 23, she said that she did not feel any knee-jerks during voluntary practice, but this does not mean that they did not occur. It is probable that knee-jerks could occur more easily after she became used to the experiment. The earlier long times are sometimes due to distraction, as she suffered from a cold until March 9. On March 7 and 8 there was considerable disturbance from a tapping experiment.

The mean variation for the RVK ranged from 9.6 to 32.1. The median on March 15 was 137. On other days it was close to the average.

TABLE III

Note: In all the following tables, the average value in sigma is given, with the number of records for each average in italics.

	RVK TO BLOW R 10°	IMITATION KJ		RVK TO BLOW ON L HAND	
		Right	Left	18°	10°
Total range	65–708	98–745	125–820		
Second series, March 6	*98* 190.5				
March 7	*128* 190.6				
March 8	*128* 181.6				
March 9	*130* 163.7				
March 12	*130* 154.8				
March 13	*150* 155.4				
March 14	*150* 135.1				
March 15	*130* 153.1				
March 16	*150* 133.0				
March 19	*133* 125.0				
		10°–32°	10°–32°		
March 20	*20* 86.8	*70* 242.2	*70* 246.1		
		2°–8°	2°–8°		
March 21	*21* 127.8	*16* 413.8	*12* 378.5		

TABLE III—*Concluded*

	RVK TO BLOW R 10°	IMITATION KJ		RVK TO BLOW ON L HAND	
		Right	Left	18°	10°
March 22	*40* 126.1			*20* 147.6	*20* 158.5
March 23	*39* 120.9			*40* 156.5	*40* 151.0

On March 20 the knee-jerks were tested while she was reading, in the order RLLRRL, following a series of RVK's. The same procedure was carried out on March 21, except that the KJ order was LRRLLR. On both days the blow speeds were the same for corresponding R and L series. On March 21, a large number of slow blows were given in sequence and in alternation with faster blows. The kicks persisted at irregular intervals even at 2°, where the records had to be omitted from the average since the chronoscope was not started. She knew that she kicked to these blows, and the experimenter suspected that the kicks occurred to those blows which happened to be noticed. She stated that on these days she succeeded much better in concentrating on her book than in the tests of March 5 and 6. Both legs failed to respond to 1 per cent of the blows of 10° to 32°. The R leg failed to respond to 21 per cent of the blows of 2° to 8°, and the L leg, to 72 per cent.

The habit or, at least, convention, of kicking the right leg appears to be the chief effect of the practice, when the frequency of kicks is compared to that of March 5 and 6, and this 'habit' transferred to the faster blows on the left leg. Whether it was a true automatism could not be determined from her confused replies to questioning after the March 20 sitting. At this time the experimenter told her in simple terms that the kicks were mostly of voluntary reaction time, and that voluntary kicks might inhibit the knee-jerk. That the results were the same on March 21, means either that she did not understand, or else that the kicks were involuntary.

For comparison with other reaction times, on March 22, reactions of the practiced leg to blows on the right hand were recorded. The same procedure was used for all numbered subjects. The hand, wearing a large work glove, was laid on a horizontal metal bar at right angles to the forearm, with the middle phalanx of the middle finger about vertical. The pendulum hit this member. The slowest blow which would cause the momentum break to act was in this case 10°. The fact that a blow of 4° to the knee was large enough indicates that the glove broke the force of the blow. In all such series, the rubber rod was left on the tendon, in order to keep the conditions as much like those of other series as possible. The faster blow used was chosen so as just not to be painful.

The order of these series was: RVK to a blow on the R leg of 10°, and to a blow on the L hand of 10°, 18°, 18° and 10°. The order on March 23 was the same except that the blows on the L hand followed the order 18°, 10°, 10°, 18° 18°, 10°, 10°, 18°. The data for all subjects are discussed after consideration of each subject. The variability in the records may have been increased by the fact that the hand changed position, varying the effect of the blow. This did not occur with the other reactors.

Subject 2m. 2m appeared to have no knee-jerks without strong facilitation, after the first few blows. He had become used to the experimental situation before January 23, when a right knee-jerk could not be obtained even with facilitation with a blow from 22°. This lack is not explained by the fact later volunteered that he had been kneeling to do carpentry, which made his right tendon sore, for he said that the blows were not painful. The effect of discomfort is at any rate doubtful, as pain has been stated to increase the knee-jerk (35) and to inhibit it if the knee-jerk follows the pain (1, 61, p. 30). He was practiced with a RVK to a blow too slow at first to elicit a knee-jerk. When the RKJ was tested it preceded the RVK series, 10 blows being given each day from February 13 to 24. The data are presented in table IV.

The m.v. of the RKJ series up to the second series of February

TABLE IV

	RKJ	LKJ	RVK TO BLOW R 22°	RVK TO BLOW ON L HAND	
				8°	12°
Total range	98–167	100–135	90–440	140–277	150–262
January 23			*101* 239.0		
	22°–30°				
January 24	*6* 129.5		*100* 202.4		
January 25			*100* 174.4		
January 26			*96* 177.6		
January 29			*100* 180.2		
January 30			*100* 182.1		
February 1			*98* 185.0		
February 2			*49* 186.2		
February 7			*101* 189.5		
February 9			*100* 190.7		
	14°–26°				
February 10	*15* 119.8		*99* 203.4		
	16°–26°				
February 12	*11* 115.5		*100* 185.5		

TABLE IV—*Concluded*

	RKJ	LKJ	RVK TO BLOW R 22°	RVK TO BLOW ON L HAND	
				8°	12°
	22°				
February 13	*4* 128.0		*100* 168.0		
February 14	*6* 124.2		*100* 170.8		
February 16	*9* 112.9		*98* 161.4		
February 20	*10* 128.4		*99* 166.4		
	22°				
February 21	*3* 122.3		*35* 180.3	*35* 181.3	*35* 183.6
February 23	*0*		*35* 173.0	*35* 173.0	*35* 182.7
February 24	*6* 124.0		*35* 174.2	*35* 183.5	*35* 181.9
	22°, 26°	22°, 26°			
February 24, 2nd series	*6* 124.2	*4* 121.5			
	22°, 26° 37°30′	22°, 26° 37°30′			
April 7	*3* 125.3	*4* 114.5	*40* 179.4		

24 was 4.3 to 12.1, and of the RVK to blow R, 9.4 to 21.8, with the exception of February 2, when it was 31.5. The subject was in poor health on this day. The m.v. decreased generally with practice. The range of the RVK records is noteworthy. Knee-jerks could be seen to occur before the RVK's sometimes, al-

though he was unaware of them, as was determined from questioning after the last sitting. Some anticipations may be included in the records. Records below 140 sigma were considered to be of knee-jerks, as there was a separation in the distributions at that point. Such records decreased in frequency of occurrence beginning February 14, as also did the RKJ on February 21 and 24. The total number of these records was 91. The proportion of 22° RKJ blows to which no response was elicited between January 24 and February 24 was 40 per cent.

On January 23, six R blows of 22° to 30° were given following the RVK series, the subject reading, but no knee-jerks were elicited. The threshold varied as follows: on January 23 and 24 occasional jerks were elicited above 22°. On February 10 the threshold was lower, but when most of the knee-jerks occurred he was working loudly and unevenly at the number work, and the threshold rose when he began working smoothly. Jerks were sometimes obtained at 14° and over, and almost always at 20° and above. On February 12 these thresholds were 2° higher. In the final tests of February 24, the threshold was not appreciably changed, no jerks being elicited below 20°.

The second series on February 24 followed a rest period during which he walked down a long hall and back. He was reading a historical book on psychology. 40 R and 40 L blows were given from 22° in the order RLLR, and then 10 each from 26° in the order RL. Blows were also given from 20° while he read aloud, with no responses recordable. The RKJ m.v. was 5.8, and the LKJ, 5.0. On April 7, a total of 60 blows was given to each leg using three speeds, in two series of order LRRL, with between them a series of RVK's to blow R. The RKJ m.v. was 1.8, the LKJ 13.0. Not enough records could be obtained to compare the two legs on either February 24 or April 7.

In all tests on 2m, knee-jerks were elicited in practically no case except when he was moving or had just made a movement, such as turning a page. This subject made himself as comfortable as possible, and probably relaxed considerably while reading. No change in the right knee-jerk due to the practice could be detected.

TABLE V

	RKJ 20°	LKJ 20°	RVK TO BLOW R 4°	RKJ WITH LVK 20°	LVK TO BLOW R		RKJ TO FIRST 10 BLOWS 20°
					20°	4°	
Total range	75–142	82–138	138–632	78–138	148–443	148–462	
January 23, 24	*40* 107.5		*40* 211.9	*40* 107.4	*40* 239.9	*40* 235.1	*10* 102.7 *
January 25, 26	*40* 111.5		*40* 220.8	*40* 102.9	*40* 201.3	*40* 230.5	*10* 103.6
January 27, 29	*40* 100.9		*40* 209.2	*40* 106.0	*40* 191.4	*40* 217.1	*10* 90.3
January 31, February 1	*40* 92.1		*40* 223.8	*40* 106.7	*40* 181.1	*40* 204.0	*10* 93.9
February 3, 5	*40* 93.1		*40* 210.9	*28* 105.9	*40* 175.3	*40* 217.0	*10* 86.5
February 6	*20* 96.2		*20* 193.2	*13* 109.4	*20* 197.7	*18* 231.2	*10* 94.8
February 7	*15* 97.9		*100* 198.1				*10* 97.5
	14°–20°						
February 9	*18* 97.9		*90* 206.0				*2* 85.0
February 12	*6* 98.2		*100* 187.2				*2* 82.5
				RVK TO BLOW ON L HAND			
	20°			12°, 14°	8°		
February 20	*10* 79.8		*96* 200.8				*10* 79.8
February 21	*10* 87.1		*35* 200.5	*35* 202.5	*35* 211.3		*10* 87.1
				14°			
February 27	*10* 87.4		*35* 190.0	*35* 206.3	*35* 209.7		*10* 87.4

* Preceded by KJ series.

TABLE V—*Concluded*

	RKJ 20°	LKJ 20°	RVK TO BLOW R 4°	RVK TO BLOW ON L HAND		LVK TO BLOW R	RKJ TO FIRST 10 BLOWS 20°
				14°	8°	4°	
March 1			*32* 197.8	*35* 198.6	*35* 202.6		
March 2	*20* 85.2	*20* 89.9	*38* 195.2	*35* 202.0	*35* 212.0		*10* 80.8 *
March 3	*15* 114.7	*21* 100.2	*20* 200.9				
	12°–18°	14°–18°					
March 2, 3	*16* 105.9	*6* 116.8					

* Preceded by VK series.

The first series on February 24 was made in the order: RKJ, RVK to a blow on the L hand from 12°, then from 8°, and to a 22° blow on the R leg.

Subject 3f. 3f did not, in November, appear to have a knee-jerk, except with facilitation, after the first few blows. She was given five sittings with a total of 544 RVK's to a R blow of 12° to 22°, of which 532 were at 16°. The pendulum bob and contact in use weighed 1052.3 grams.

There was no practice effect visible in the records after the first two days. From December 14 to January 22 there were no sittings. On January 22, while she read a novel, the R knee-jerk could be easily obtained with the pendulum bob and contact weighing 430.2 grams. There is no explanation for the appearance of the knee-jerk, and it may rather be considered an effect of the vacation than of the previous practice. Records were taken, chiefly with a 20° blow, at 15-second and 20-second intervals. In the first series, January 23, the R threshold was determined first with the heavier hammer and then the lighter, and was found to be the same, 16°, for both series, except that not quite so many knee-jerks were elicited in the second series. The appearance of the knee-jerk was therefore not due to the use of a lighter hammer.

Threshold tests were made on January 22 as well as 23. The RKJ to blows of 14° to 26° averaged 106.6 sigma for 94 records. Forty-one per cent of the blows elicited no recorded KJ. On January 23, a second series was made, shown in table V. The effect of the voluntary practice of January 23 and 24 should be negligible in a KJ series on January 25, taken in the order LR, with 8 blows of 14° to 22° to each leg, while she read. The averages, not shown in table V, were: 7 LKJ, 89.7 sigma; 7 RKJ, 103.0; the m.v.'s both 2.9. The thresholds were the same, there being no records made below 16°.

The R-L difference is not entirely due to the order of the series, for in other, RKJ, series at the beginning of a sitting the difference between the averages of groups of records in positions in the series corresponding to these R and L records was only 1.0, 3.0, 6.8 or 7.9. The net difference, by subtracting the average of these, would be 8.6.

The average RKJ became quicker and less variable than the RKJ with LVK from the January 27–29 average onward. The RKJ m.v. was 5.4 to 10.1 for the six averages to Feb. 7, and the m.v. for the RKJ with LVK was 8.8 to 12.3. The proportion of RKJ blows to which no response was recorded in these 11 sittings was 4 per cent, and of RKJ with LVK, 45 per cent. The RKJ with LVK practically disappeared on January 26 and February 5, and became infrequent on February 6. The LVK to a 20° blow was usually faster than that to a 4° blow, but this difference was not marked. As she did not kick as hard as she could, the following variations are not surprising. The m.v. of the RVK to blow R on these days was 19.3 to 30.9; of the LVK to a 20° blow R, 10.7 to 33.5, but usually less than that for the RVK to blow R; and of the LVK to a 4° blow R, it was 12.9 to 36.7. The LVK to both blows decreased in variability with practice, while the RVK did not show so much decrease, as it had been practiced previously.

During practice beginning February 7 the knee-jerk series always came first. No significant change in the RKJ appears in these series. The threshold was found on February 9 and 12 to be still 16°, with, on February 9, occasional jerks elicitable at

14°. Averages of RKJ's occurring to the first ten blows in sittings when tests of them were made first beginning with January 23, with a 20° blow, are given at the right of the table. The changes are as likely to be due to the time of day, early afternoon giving the quickest times, as anything else. The average time may vary continuously from day to day, without any apparent cause. No trend ascribable to the practice is visible.

On March 2, a second series of tests was made of knee-jerks in the order RL, ending with 10 20° blows in the order LR, with the subject reading. The threshold for the RKJ was slightly lower than that for the LKJ, but this difference is plainly due to the fact that the RKJ threshold test was made first. The same is true of the averages. On March 3, the R and L thresholds were tested immediately after the RVK's, while she read. No right involuntary kicks to a blow of 4° or more were obtainable. The knee-jerk series followed in the order LRRL. In this series the RKJ was slower than the LKJ, and the m.v.'s were 10.7 and 9.2 respectively. The RKJ was also less frequent, there being 52 RKJ blows and 54 LKJ blows, with fewer RKJ records than LKJ. The infrequent, and high records are due to the series coming late in the sitting. The R-L difference is greater than the net difference of January 25. The arrangement of the series prevented the testing for any difference immediately following a series. What is seen is possibly a lasting depression due to the practice.

Additional records were made as shown in table V, on February 21 and 27, and on March 1 and 2, when the order of the voluntary kick series was the reverse of that in the table. She said that the 8° blow was so light that it touched only the glove.

Subject 4m. Records taken on 4m on February 2 were preceded by a series to determine the RKJ threshold. No jerks were elicited below 18°, and they usually occurred at that speed and above. Comparison series were made on six days, for which the data are in table VI. No practice effect is visible. For an unknown reason the LVK to blow R became slower, that to a 20° blow becoming more variable. The m.v. of the RVK to a 4° blow R was lowest for the February 9–12 average, and ranged

TABLE VI

	RKJ	LKJ	RVK TO BLOW R 4°	RKJ WITH LVK 20°	LVK TO BLOW R	
					20°	4°
Total range	80–155	87–162	125–285	90–157	98–273	133–230
	20°					
February 2, 3	*29* 103.5		*40* 180.3	*25* 114.8	*40* 143.6	*40* 166.4
February 5, 6	*31* 113.2		*40* 183.6	*18* 118.2	*39* 145.0	*40* 164.6
February 9, 12	*28* 105.8		*40* 184.4	*18* 125.3	*40* 178.6	*40* 178.0
	20°–28°	22°–28°				
March 5	*12* 126.6	*12* 128.4				
	18°–32°	18°–32°				
March 6, 7	*97* 110.7	*75* 119.0				
			RVK TO BLOW R			
			14°	16°		
Total range			85–350	103–265		
March 8			*60* 161.1	*42* 146.7		
March 9			*100* 164.5			
March 12			*100* 161.0			
March 13			*100* 164.2			
March 14			*99* 156.2			
March 19			*101* 151.3			

TABLE VI—*Concluded*

	RKJ	LKJ	RVK TO BLOW R		LVK TO BLOW R	
			14°	16°	20°	4°
	18°-30°	18°-30°				
March 20			*20* 149.7			
March 21	*108* 109.7	*80* 118.8	*20* 152.5			
March 22			*20* 157.1			
				RVK TO BLOW ON L HAND		
				6°, 8°	18°	
March 23			*40* 157.2	*40* 175.3	*40* 163.4	
				8°		
March 24			*40* 148.0	*40* 160.4	*40* 152.2	

from 18.0 to 24.7. For the LVK to a 20° blow R the m.v. was 9.1 to 24.5, and for the LVK to a 4° blow R it was 13.9 to 14.9. The m.v. of the RKJ to 20° was 9.9 to 13.0, and of the RKJ with LVK, 7.0 to 11.3. The m.v. of the RKJ with LVK was less than that of the RKJ except for February 5–6. This is probably due to the fact that RKJ series averaged were from the beginning and end of their respective sittings, while the RKJ with LVK were not. The RKJ with LVK was slower than the RKJ. It practically disappeared on Febrary 3, 6, 9 and 12 and became very infrequent on February 5. There was no response recorded to 72 per cent of the RKJ with LVK blows, and to 50 per cent of the 20° RKJ blows. This difference would appear larger if more blows had been given at a time, as the RKJ never became very infrequent. The RKJ with LVK was definitely and increasingly slower and less frequent than the RKJ alone. The LVK to a 20° blow R was faster than the RVK to a 4° blow R, perhaps because of the greater amount of practice of the LVK.

The thresholds were determined on March 5 with 24 blows to each leg, 4 at each blow speed tried. Jerks were elicited sometimes at 20° for the right leg, and 22° for the left, and usually at 24° and over for both. As the order was RL, the difference between the two legs is negligible.

On March 6, a series of RKJ's with 39 blows was made, but is not reported because there was apparatus difficulty. The tension on the cord from the ankle had to be lessened from this time on, as otherwise the heavier blows broke the contact that it controlled. The series reported was taken in the order RLLR. The order in which the blows of different speed were given was partially equated for the two legs to equalize temporal effects for the blows, and as usual the same number of each speed of blow was given to each leg with one exception. The series on March 7 was in the order LRRL and the blow speeds were mixed in a series of 18 which was repeated in reversed order. There was no response to 25 per cent of the RKJ blows on these two days, and to 41 per cent of the LKJ blows. The m.v. of the RKJ was 11.5, and of the LKJ, 15.6. A consistent difference is visible in the reaction times, frequency and variability.

Practice was given of the RVK to a blow R at first definitely too slow to elicit a knee-jerk. They appeared before the voluntary kicks during practice, however, even after the blow was lowered from 16° to 14°. Only 2 blows were given from 16° on March 9. As a dividing line appeared in the distributions of records at 133 sigma, the records below this figure were counted as knee-jerks. There were 11 of these in the series at 16°, and 21 the rest of the time, with no visible practice effect. The greatest frequency was 9 on March 19. Some slow decrease in the RVK records is visible.

On March 20, 21 and 22, KJ series were taken following the RVK series, and were in the order RLLR, LRRL and RLLR. The order of the blow speeds was arranged to equalize temporal effects, the series on March 21 and 22 being like those of March 7. The R leg failed to respond to 27 per cent of the blows, and the L, to 46 per cent. The LKJ was still the slower of the two and less frequent. The mean variation was 9.6, and that of the

RKJ was 11.1. The series of March 21 and 22 were more regular. They averaged 109.8 for the RKJ, and 117.8 for the LKJ, with medians of 107 and 118, and m.v.'s of 11.5 and 9.2 respectively. The RKJ was not recorded to 23 per cent of the blows of 18° to 30°, and the LKJ to 47 per cent, the RKJ threshold being lower by 2°. The m.v.'s were doubtless influenced by the thresholds, as more R records were obtained with slower blows, and some correlation is visible between blow speed and reaction time.

The difference between the RKJ and LKJ appears to be the same as before practice. If the first few blows following a practice series is considered, the difference is the same.

Records were taken on March 23 in the following order: RVK to a blow of 14° on the R leg, then to 6°, 18°, 18°, 8°, 8°, 18°, 18°, 8° on the L hand. On March 24 the order was: RVK to 14° on the R leg, then to 18°, 8°, 8°, 18°, 18°, 8°, 8°, 18° on the L hand.

Subject 5f. From December 13 to January 19, 5f had practiced the LVK to 115 blows on the R leg from 12° and 16°, and to 230 from 18° and 24°. She practiced the RVK to 115 blows on the R leg from 12° and 16°. A number of other blows were also used in knee-jerk tests. The blows from 12° and 18° were given with the pendulum bob and contact device weighing 1052.3 grams. The other blows were given on January 18 with the bob and contact device weighing 430.2 grams. The apparatus was in its final form on January 19, and records are reported from that day on.

Data from subject 5f are in table VII. It will be noticed that the LVK to 26° R is quicker than either the LVK or RVK to a 4° R blow. This difference was visible from the first day of practice and is therefore not a practice effect. The RKJ is quicker and less variable than the RKJ with LVK. The RKJ m.v. was 6.4 to 19.4; and the m.v. of the RKJ with LVK was 6.0 to 13.0 up to February 12. For the RVK to blow R, 4°, the m.v. was 9.9; for the LVK to blow R, 26°, it was 5.7 to 18.0; and for the LVK to blow R, 4°, 7.8 to 15.0, which was mostly higher than that for the LVK to blow R, 26°.

The RKJ with LVK occurred with continually decreasing

TABLE VII

	RKJ 26°	LKJ 26°	RVK TO BLOW R 4°	RKJ WITH LVK 26°	LVK TO BLOW R		LVK TO BLOW ON R HAND	
					26°	4°	16°	8°
Total range...	70–143 (315)	73–140 (310)	155– 213	73–138 (243)	110– 642	130– 283		
January 19, 23, 24, 25....	*80* 91.4		*80* 178.6	*80* 98.9	*80* 140.9	*80* 176.1		
January 26, 27..........				*63* 103.5	*70* 149.6	*70* 171.8		
February 2, 5.				*60* 96.1	*70* 144.5	*70* 182.0		
February 7, 9.				*40* 111.9	*55* 139.1	*55* 170.5		
February 10, 12..........				*37* 88.9	*70* 141.9	*69* 178.1		
February 13, 14..........				*70* (105.3) Med. 100	*70* 148.6	*70* 181.0		
February 15, 16..........				*70* (109.5) Med. 103	*70* 148.2	*70* 178.2		
February 27...				*35* (114.7) Med. 98	*35* 148.3	*35* 185.7		
February 28...	*15* 99.6 Med. 98 & *12* (264.1)	*33* 93.4 93 & *6* (293.0)		*9* (111.6) Med. 90	*10* 134.0	*10* 170.3		
March 2......	*34* 98.8 Med. 97 & *1* (277)	*36* 103.1 98		*19* (101.3) Med. 102	*10* 150.9	*10* 205.3		

TABLE VII—*Concluded*

	RKJ 26°	LKJ 26°	RVK TO BLOW R 4°	RKJ WITH LVK 26°	LVK TO BLOW R		LVK TO BLOW ON R HAND	
					26°	4°	16°	8°
March 3				*19* (112.8)	*20* 145.6	*20* 182.0	*50* 158.0	*50* 162.6
March 28:								
First series	*21* 103.6 Med. 100	*30* 99.7 98		*19* 139.7	*20* 147.1			
Second series	*5* 95.2 Med. 102	*21* 98.6 92						
Both series	*26* 102.0 Med. 100	*51* 99.2 95						

frequency. It became impractical to obtain the desired number of records in each sitting. This response became infrequent on January 27, February 2, 7, 9 and 14, and practically disappeared on February 10. The proportion of RKJ blows to which no response was recorded for subject 5f is exactly 0 per cent; of RKJ with LVK blows, 48 per cent; up to February 14. This percentage is not reliable for any subject, for when the frequency decreased so much that the desired number of records was not obtained with twice that number of blows, the series was discontinued, while with prolongation of the series the proportion of records would have decreased further.

From February 14, a right leg response appeared, which was sometimes indistinguishable from the knee-jerk, but ranged to kicks with obviously voluntary reaction times. Data including these are indicated in parentheses. Nine records between 277 and 312 sigma were obtained on January 24 and 25, but were not identified as 'imitation knee-jerks' because no kick at all was visible. She remarked on these kicks, and said they were probably voluntary to some extent, since she felt that the right leg ought to be responding. She probably did not recognize very small knee-jerks. On February 16 this 'imitation jerk' response continued into the series to a 4° right blow, and she apologized. The disappearance of the knee-jerk apparently did not seem to her a necessary accompaniment of the contralateral kick. In this respect she differed from subject Ef.

On February 28, the voluntary kick records were taken first, in the order shown in table VII. Then, while she was reading an illustrated book, 4 test blows from 4°, 2 from 8°, and 4 from 26° were given to the right leg, with recording from the left. No responses of the left leg were elicited. Two blows from 4° and 1 from 8° were then given the right leg with recording from the right leg, with no response.

The knee-jerk series followed, in the order RLLR. On March 2, the records of the RKJ with LVK were taken first, then 15 RKJ records, the first 4 alternated with slow blows to see if imitation knee-jerks would occur to the slow blow; they did not. For a distraction on this day she read a popular psychological book. The LVK records to a 4° R blow and to a 26° R blow were then taken, followed by 8 26° R blows with recording from the L leg while she read. No left kicks occurred. Just before the last blow, the experimenter asked if the breeze was too strong. She guessed why the question was asked, and the distraction had no effect. The rest of the KJ records were taken in the order 10R, 35L, 10R. The records for February 28 and March 2 are reported separately because of the difference in the voluntary reaction times. Some medians are given for this subject because they did not fall close to or just below the averages.

In these series the 'imitation knee-jerks' occurred to both the LKJ and RKJ blows, although no contralateral kicks were being made. Where they were not preceded by KJ's they were clearly distinguishable by their reaction times, and are therefore separately listed. Those of the L leg, however, were less frequent and occurred only on February 28. The RKJ with LVK was no slower than the RKJ, except that the average was lengthened by the imitation jerks. The reason assumed for this fact is that the KJ series were extended, while those with the LVK were not, and relaxation or whatever occurs with time could not take effect.

The LKJ on February 28 was quicker than the RKJ. This may be laid to an actual effect of the voluntary practice. Such an effect was soon lost after prolonged elicitation of knee-jerks. On March 2, the LKJ series was begun at least 20 minutes after the RKJ series was. The longer times that increased the average are thus accounted for. The medians are more significant.

On March 28, she was again tested, the distraction being a book on psychology. The RKJ and LKJ were tested under well equalized conditions. The first series, of order RLLR, was followed by a rest period, during which she walked down a long hall and back, then by the series of the LVK to blow R, and the RKJ with LVK. The second series of KJ's was also of order RLLR, although no jerks could be obtained in the second group of R blows. The 'imitation knee-jerk' did not seem to occur in response to the KJ blows.

The LKJ reaction times on this day were shorter than the RKJ, and the difference increased after practice, as seen from the medians. The reason that the RKJ average was less than the LKJ in the second series may be that there were too few records to be reliable. If the first few R and L blows in test series following LVK practice are considered, the result is more obvious. On February 28, following the LVK series 8 RKJ records were obtained in 20 blows, averaging 104.1 sigma, and then 13 LKJ records in 20 blows, averaging 94.8 sigma. The medians were 97.5 and 95. On March 2, 9 RKJ records in 12 blows averaged 93.0 sigma, and 8 LKJ records in the next 12 blows averaged 114.0, the medians being 93 and 115. On March 28, following the LVK practice, 5 RKJ's in 16 blows averaged 95.2 sigma and 11 LKJ's in the next 16 blows averaged 97.7, the medians being 102 and 92. The medians are more significant for brief series and indicate here a definite difference, except on March 2, when there had been recent extended testing.

The order of series on March 3 was: 10 LVK's to 4° R, 10 to 26° R, then those to a blow on the R hand from 16°, 8°, 8° and 16°; 10 to a 26° blow R, 9 to a 4° blow R, and then the RKJ's with LVK. During the last series she was particularly attempting to concentrate on the left leg, and right 'imitation jerks' were still visible. Test blows were given at the ends of the last three series. A blow at 4° she noticed was not preceded by a ready-signal, and she did not kick. One at 26° she could not remember afterwards, but she had kicked after 303 sigma. To 3 others, eliciting recorded RKJ's, she gave 2 left kicks. She remarked, after kicking in response to one of the last ones "Oh, you didn't say 'ready'." She said that the kicks she made were not just like those when there had been a ready-signal. These kicks appear to have been automatic, but of voluntary origin, and not similar to reflexes.

The right 'imitation jerks' did not occur when the blow was on the hand. This fact indicates at least that she did not think the leg should move. Certain 'imitation jerks' occurred without her knowledge. On one occasion she expressed surprise at one, when they had been occurring right along. They may be characterized as responses to KJ blows when the subject was not distracted by reading but was presumably paying attention to the other leg, except on February 28 and March 2, when they were carried over temporarily to the reading situation and for a briefer time to the left leg. Although they seemed to originate because of a belief on the part of the subject that kicks should occur, after a time they became automatic.

Subject 6f. 6f reported morning and afternoon at regular hours, and received the most frequent practice of any subject. In the first three sittings, of March 5 and March 6, a.m., records were taken while she was reading. Except for the first ten blows on the R leg, the speeds were ordered to equalize temporal effects. Some blows from 8° and all from 22° were added, mostly at the end of the series. The right and left series were arranged as follows for these sittings: RLLRRL, RLLR and RLLR. 'Imitation knee-jerks' were invariably obtained when no knee-jerk occurred, the reaction times being distinctly longer, and some were seen to follow knee-jerks. The average of 63 R and L 'imitation jerks' was 321.8 sigma, and the range 165 to 642. Records of these for which no movement was visible are not reported. The other records are in table VIII.

The proportion of RKJ blows from 20° to which no KJ occurred was 3 per cent, and of LKJ blows, 6 per cent. Considering all blows from 10° to 20°, totaling 200 and 204 respectively, the percentages were 43 for the RKJ blows and 33 for the LKJ. The frequency with which KJ's occurred is high for this subject.

TABLE VIII

	RKJ 10°–20°	LKJ 10°–20°	RVK TO BLOW R 4°	RKJ WITH LVK 20°	LVK TO BLOW R		LVK TO BLOW ON R HAND	
					20°	4°	8°	18°
Total range..........	67–127	68–132	143–323	68–130	112–383	130–483		
March 5; 6, a.m....	*113* 91.4	*135* 91.8						
	20°							
March 6, p.m........	*15* 77.6		*15* 209.5	*15* 82.4	*15* 181.1	*15* 193.6		
March 7, 8, 9, 12...	*239* 79.3		*229* 203.9	*228* 92.6	*230* 161.4	*230* 197.4		
March 13, 14.......				*140* 91.9	*200* 161.4	*200* 198.9		
March 15, 16.......				*148* 95.3	*198* 160.7	*199* 195.8		
March 19, 20.......				*154* 91.7	*200* 166.0	*200* 200.9		
		20°						
March 21, 22.......	*197* 93.0	*200* 84.7			*80* 154.4			
March 23, a.m.......					*80* 163.7		*40* 206.7	*40* 193.3
March 23, p.m.......							*40* 194.1	*40* 186.9

The RKJ median was 88 and the LKJ 83, but there was no difference in the averages, and the m.v.'s were 13.5 and 14.5 respectively.

The order of the comparison series begun March 6, p.m., was reversed on successive days. No practice effect appeared after March 6. The RKJ m.v. was 3.9 for the short series of March 6, and 4.7 for the average of the next 8 sittings. The m.v. of the RKJ with LVK was 3.1 and 12.3. This response was slower than the RKJ and became more variable. The m.v. of the LVK to 20° R was 10.0 and 17.5; of the LVK to 4° R, it was 6.6 and 25.7; and of the RVK to 4° R, 10.2 and 18.4. The LVK to 20° R was the quickest voluntary kick, that to 4° R the next, and the RVK to 4° R the slowest. The last did not show any relative slowing between March 7 and 12 due to more practice in the L kicks.

Further changes appeared in the RKJ with LVK when only the L kicks were practiced from March 13 to 20. Its m.v. was 13.2 to 13.5, but this kick became very infrequent on March 14, a.m., 15, a.m., 16, a.m., and 20, p.m., and practically disappeared on March 14, p.m., and 15, p.m. The proportion of blows to which no response was recorded was 34 per cent. (The a.m. and p.m. records of the RKJ with LVK were compared for the March 7–8–9–12, 13–14, 15–16 and 19–20 averages. The p.m. records were 3.5 to 5.7 sigma less or on the average 5.0. The percentage of blows to which no response was recorded was in the a.m., 32, and p.m., 24.) The m.v. of the LVK to 20° R was 15.0 to 16.2, and of the LVK to 4°R, 15.4 to 16.6.

On March 21 the order was: LVK to blow R, 2 test blows from 20° and one each of 26° and 30° with recording still from the left leg, while she read. A KJ series followed in the order RLLRRLLR. On March 22 the order was the same except that the KJ series was in the order LRRLLRRL. The LKJ was definitely faster in each of the 4 sittings than the RKJ, by a difference greater than the original difference between the medians on March 5 and 6, and the m.v. of the LKJ was 8.9, and of the RKJ, 10.3, although the LKJ m.v. was the greater for two of the sittings. There was no response to less than .5 per cent of the LKJ blows, and to 3 per cent of the RKJ blows. No 'imitation knee-jerks' occurred. The difference between the two legs was greatest on March 21, a.m., when the RKJ average was 98.5, and the LKJ, 83.4. The first 10 RKJ on March 21, a.m., averaged 100.4 sigma; p.m., 85.3; March 22, a.m., 89.0; and p.m., 85.8. The first 10 LKJ averaged, in the same sittings, respectively, 73.5, 78.4, 80.5 and 79.3. There was a transitory large difference, due to the practice, which disappeared with extended testing and also a more permanent difference which shows in the averages after March 21, a.m., and which is larger than that originally visible in the medians.

On March 23, a.m., the following series was made: LVK to 20° R, then to 8°, 18°, 18°, 8°, 8°, 18°, 18°, 8° on the L hand.

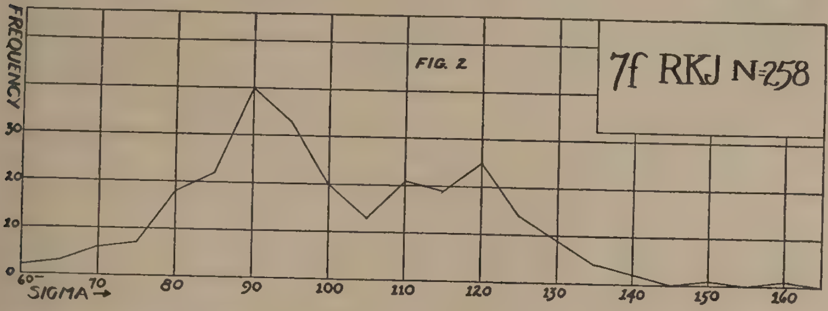

FIG. 2

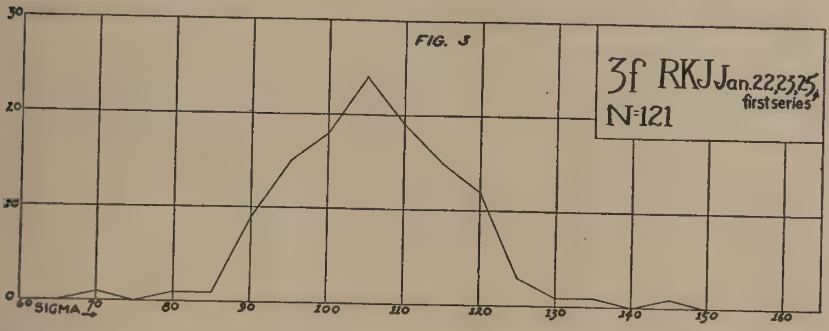

FIG. 3

The p.m. series was the same except that the blow speeds to the hand were in the order 18°, 8°, 8°, 18°, 18°, 8°, 8°, 18°.

Blow speed and reaction time. The knee-jerk data in tables IV, VI and VIII can be averaged to show the effect of blow speed. When this is done, using approximately the same number of records at each blow speed, a decrease is visible in the knee-jerk reaction time with increase in blow speed above a certain value for each subject. The change is not, for these few cases, at all regular.

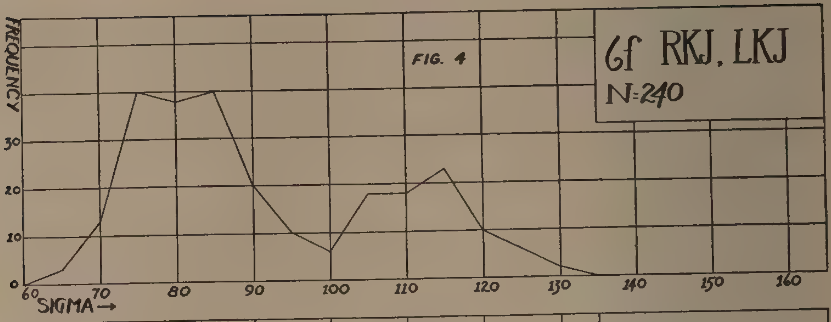
FIG. 4

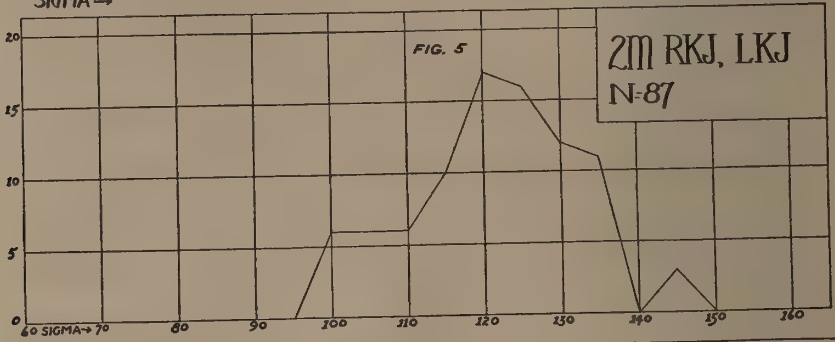
FIG. 5

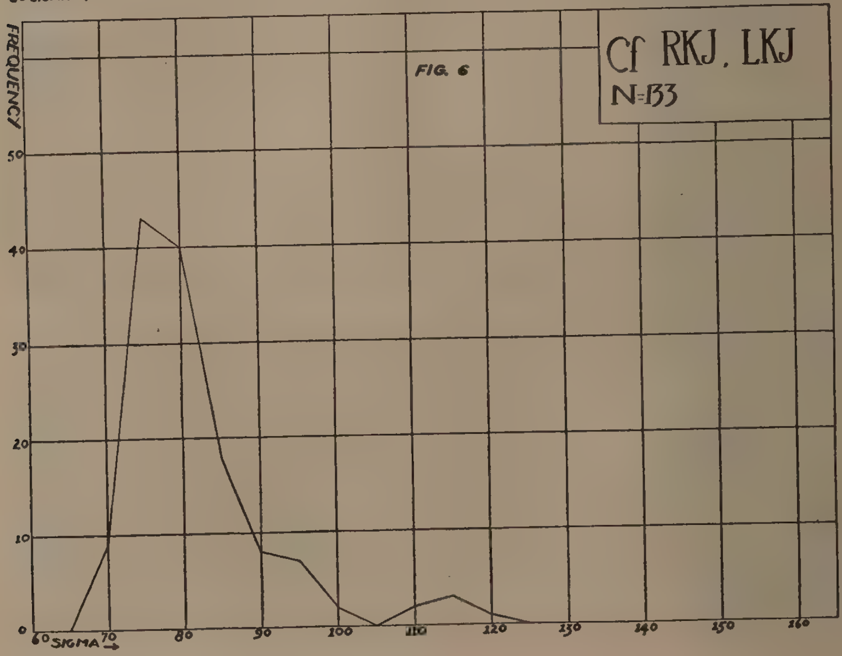
FIG. 6

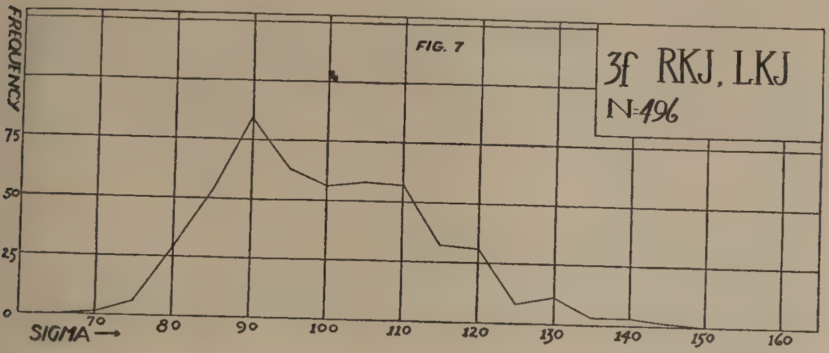
FIG. 7

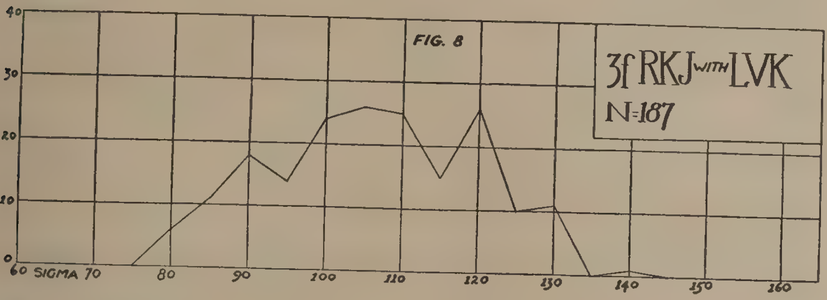
FIG. 8

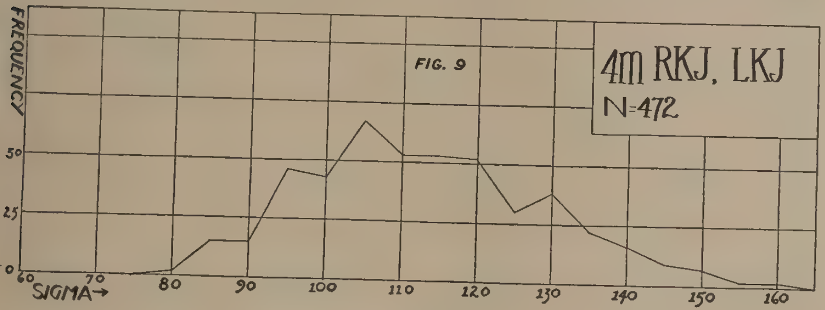
FIG. 9

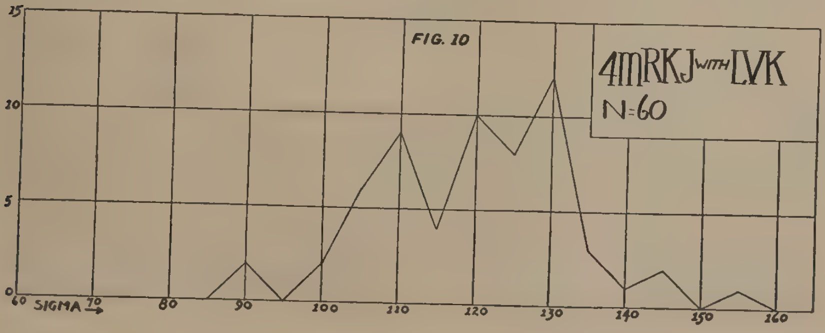
FIG. 10

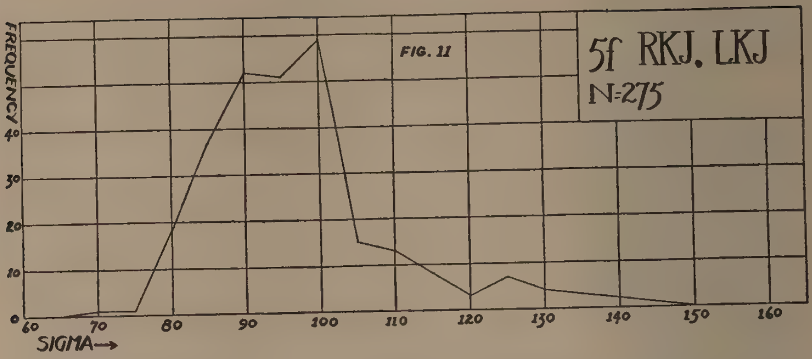
FIG. 11

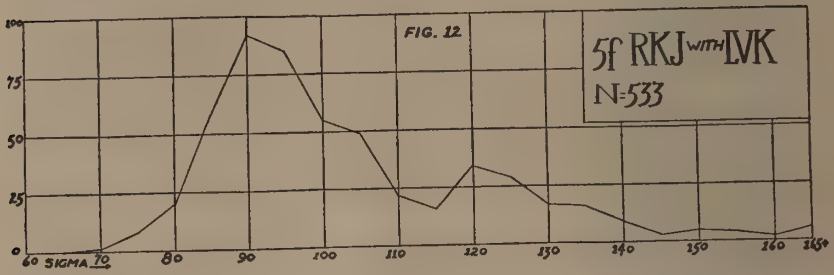
FIG. 12

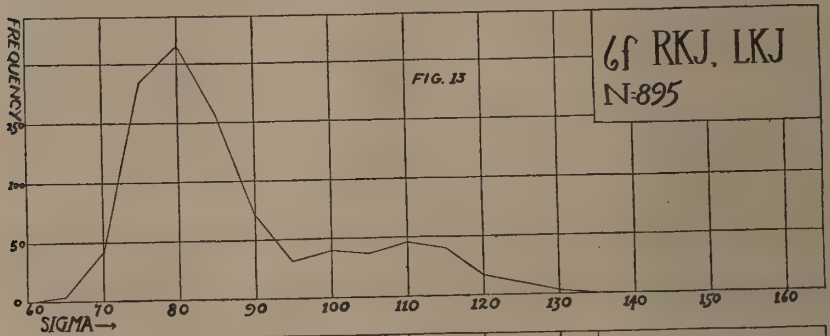
FIG. 13

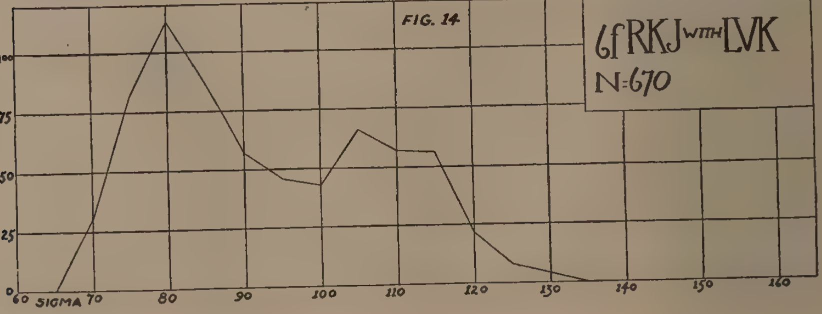
FIG. 14

The relation between the weight and speed of the blow does not appear to be the same for all the subjects. The weight of the pendulum did not much affect the speed necessary for 3f and Cf. For 5f, a much faster blow was necessary with a lighter pendulum bob.

TABLE IX

SUBJECT	PRACTICED RESPONSE	M.V.	VK TO SLOW BLOW ON HAND	M.V.	VK TO FAST BLOW ON HAND	M.V.	ALL VK TO BLOW ON HAND	M.V.	NUMBER OF RECORDS IN SERIES
1f	*79*		*60*		*60*		*120*		*20, 40*
	123.6	11.4	153.5	21.2	153.5	17.8	153.5	19.5	
2m	*103*		*105*		*105*		*210*		*35, 35, 35*
	177.1	10.8	179.4	15.4	182.8	12.5	181.0	14.2	
3f	*140*		*140*		*140*		*280*		*35, 35, 35, 35**
	195.9	15.7	208.9	18.7	202.4	14.2	205.6	16.5	
4m	*80*		*80*		*80*		*160*		*40, 40*
	152.6	11.2	167.8	9.2	157.5	16.4	162.8	17.6	
5f	SLOW BLOW								
	19		*50*		*50*		*100*		*50*
	182.0	9.4	162.6	18.4	158.0	13.5	160.3	16.2	
	FAST BLOW								
	20								
	145.6	5.7							
6f	*80*		*80*		*80*		*160*		*40, 40*
	163.7	11.6	200.4	21.1	190.0	18.4	195.2	20.3	

* Approximate.

Distributions. The knee-jerk records of each subject are placed together in distributions in figures 2 to 14. Some show definite bimodality, which is particularly evident in the reaction times of the knee-jerk accompanying a voluntary kick of the opposite leg. In some, there is a suggestion of polymodality.

Figure 4 includes only records from March 5 and 6, a.m.

The first three distributions show that the bimodality is not necessarily due to any of the following procedures: combining RKJ and LKJ distributions having different central tendencies, combining distributions from one leg which were obtained respectively at the beginning and end of a sitting, with no records from the middle, or including RKJ records after practice of an inhibiting response. The distribution from 3f, however, compared to the two later shown from that subject, suggests that the polymodality visible may be due to such factors.

Data with hand stimulation. Table IX contains the averages obtained for comparison of reaction times to a blow on the knee-tendon with those to blows on another member; the opposite hand. For subjects 1f, 2m, and 4m, the corresponding series of practiced responses consisted of the RVK to a blow that in practice had sometimes elicited knee-jerks (cf. table II). For 2m, the records below 140 sigma are omitted. The practiced response of 3f was a RVK in response to a very slow right blow 5f and 6f had practiced the LVK to a R blow of knee-jerk speed. Their records are of a LVK, therefore. Records are shown for 5f, also, of the LVK to a very slow R blow. The response to a fast blow on the opposite hand was quicker than that to a slow blow, except for 2m; but it was not quicker than the response to a slow blow on the same leg, in subject 3f. The response to either blow on the opposite hand was consistently slower than any practiced response, and this difference was not altered by continued practice of the response to the hand blow (cf. tables IV and V).

VII. CONCLUSIONS

1. It appears that the knee-jerk can be altered by practice in voluntary kicking in response to the knee-jerk stimulus. If one leg is percussed during practice, the effect of the voluntary response is in accordance with the type of response.

(a) If the percussed leg is voluntarily kicked backward in practice, the knee-jerk time of the percussed leg is increased relative to that of the opposite leg during practice and 10 to 20 following test blows.

(b) If the contralateral leg is kicked forward voluntarily in practice, the knee-jerk time of the contralateral leg is decreased relative to that of the percussed leg during 10 to 20 blows following.

(c) If the percussed leg is kicked forward in practice, there is little if any effect on the knee-jerk.

2. The voluntary reaction time of the contralateral leg is shorter, when the stimulus is a blow of knee-jerk strength, than to a tactual stimulus over the tendon, or to a tactual stimulus elsewhere. So also is the reaction time of the leg to a sub-KJ blow on that leg less than that to a tactual stimulus elsewhere.

3. After practice in contralateral kicking, or in back kicking of the percussed leg, the corresponding response to an unexpected tendon blow of knee-jerk strength may occasionally occur involuntarily. The degree of habituation of this response is greater in persons who are interested or who believe learning is expected.

4. Voluntary forward kicking of the contralateral leg to a knee-jerk stimulus tends to depress or abolish the knee-jerk in the percussed leg.

5. The frequency distribution of knee-jerk reaction times is bimodal in subjects exhibiting a lively knee-jerk. The bimodality is increased when a contralateral voluntary kick is made to knee-jerk stimulus.

6. The increasing of the strength of the knee-jerk stimulus by increasing the speed with which the hammer strikes the tendon seems to present three ranges. (*a*) A threshold range, within which the knee-jerk may or may not be elicited, (*b*) a second range, within which the several strengths have approximately equal effects and (*c*) a third range, in which the knee-jerk time is appreciably shortened, as compared with the second range.

These conclusions are tentative, the exploratory nature of the investigations, with necessary scattering of the data, not permitting final conclusions. The precise points on which further data need to be gathered are, however, made clear. It is obvious that the knee-jerk, in the normal individual, is no simple segmental 'reflex', but an integrated response, of a complex nature.

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Food-Tokens As Incentives For Learning By Chimpanzees

BY

JOHN T. COWLES

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FOOD-TOKENS AS INCENTIVES FOR LEARNING BY CHIMPANZEES[1]

JOHN T. COWLES

From Yale Laboratories of Primate Biology

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FOREWORD BY ROBERT M. YERKES

In laboratories of psychology it is common practice to rely on the need of animal subjects for food almost to the exclusion of other possible forms of motivation, whereas in the case of human subjects the opposite is true. This difference may be unfavorable to the comparability of results for man versus other animals. For this reason, and also on account of convenience, economy, nutritional hygiene, dietary regimen, and the importance of extending motivation to higher psychological levels than those of common physiological needs, it would seem important to attempt to extend the range of experimental motivation for animal subjects. In the Yale Laboratories of Primate Biology this is being attempted for the chimpanzee by tests of the value, dependability, and measurability of various motivating conditions distinct from, or only indirectly related to, hunger.

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[1] An abridgement of a dissertation presented to the faculty of the Graduate School of Yale University in candidacy for the degree of Doctor of Philosophy. Reported in part before the American Psychological Association at Poughkeepsie, N. Y. (Eastern Branch), April 3, 1937, and at Minneapolis, Minn., September 1, 1937. The writer is greatly indebted to Professor Henry W. Nissen, who throughout the investigation gave untiring guidance and friendly criticism, and to Doctor John B. Wolfe, who made useful suggestions based upon closely related experimentation.

Among the possibilities which are receiving attention are the presentation of an object which has come to be strongly desired or treasured by the subject; entertainment reward, in the shape of stimulus-producing mechanisms which may be operated by the subject; social rewards, such as companionship, exercise, social play, commendation or praise by the experimenter; or, the utilization of a token or symbolic incentive, whose value depends upon trained association with food or with some other motivating condition.

Doctor Cowles offers in this report a significant contribution to the general subject of token incentives. His study supplements Wolfe's (1936) previously reported investigation concerning the value of tokens. It has now been established that this motivational procedure may be used advantageously for chimpanzee in certain types of experiment.

I. INTRODUCTION

It has long been recognized that what are loosely known as motivational conditions play a leading part in the determination of organismic behavior. Whether an animal is hungry, and whether a given act is followed by food or not, are of critical importance for future behavior in the same or similar situations. Although there is much difference of opinion as to how and why such motivational factors operate, concerning the fact itself there seems to be little doubt.

Perhaps the majority of investigations which have been conducted in this field with animal subjects have been primarily descriptive or normative in nature. Comparisons have been made of various kinds and amounts of deprivation and of different kinds or quantities of incentive or reward. The analytical approach, the attempt to discover the "how" or the "why" of motivational factors, has been represented more by rationalistic and speculative than by experimental methods. Studies of the temporal relations between various activities and their biological significance to the organism have thrown some light on these questions. A different but probably related approach to the problem is found in several investigations of the efficacy of indirect rewards, associated with more obviously useful or direct goals.

Williams (1929) showed that a rat will improve its scores in running a maze when the immediate goal is the food-associated stimulus of a previously established visual discrimination habit. McCulloch (1937) has reported a case of visual discrimination learning by an infant chimpanzee, where the incentive was an object (paper towel), previously neutral or negative, but which had been associated with the highly preferred activity of running to the experimenter's arms. In a number of recent studies the general method has been to employ, as incentives, objects which only by certain associative training with the primary goal have acquired effectiveness in eliciting native or previously learned responses. Thus Wolfe (1936) has shown in these Laboratories that discs, to which the chimpanzee is at first quite indifferent, may be experimentally assigned any of a wide range of incentive values, by means of associative training with certain food-rewards or goals of preferred activity, and has demonstrated that the chimpanzee will then perform work for these tokens. The food-tokens serve as incentives not only for single work-responses, but may also elicit a succession of these responses before the tokens received for the work are exchangeable for food. Wolfe (1936), and Nissen and Crawford (1936) have used similar food-tokens to elicit certain types of social behavior in chimpanzees. Ellson (1937) has trained dogs to exchange small rubber balls for food, and has then used the ball as incentive for the performing of a simple work-habit, the depressing of a lever which releases the balls to the animal. Mitrano (1937) has trained feeble-minded humans to exchange "poker-chips" for food and then has used these chips to elicit the insertion of marbles into a device which delivers the chips to the subject. In both of these last two experiments the habit sequences were maintained with the immediate exchange of each single ball or chip for food.

It will be noted that, with the possible exceptions of Williams' and McCulloch's experiments, these studies involved the performance of an already learned habit or native response. Relatively few experiments have utilized the *acquisition of new habits*, instead of performance of old, as the function observed under the varying motivation. Such experiments have almost exclusively involved direct reward or satisfaction of the animal's need.[2] It is probable that the immediate goals of most human learning, however, are but indirect means to, or substitutes for, the satisfiers of the fundamental bodily needs which give incentive value to those otherwise indifferent goals. More experimental data are obviously needed on this problem of substitute rewards in learning, especially since, as will appear from our later discussion, the significance of indirect rewards may be more fully and clearly evident when these lead to the formation of *new* habits than when they merely promote repetitions of previously established responses. It was to this end that the present investigation was designed.

With Wolfe's experiments as the immediate point of departure, the investigator trained chimpanzees to associate previously indifferent (behaviorally neutral) objects with a certain food-reward. It was then determined whether these tokens would operate as incentives for the learning of standard laboratory problems. These problems were simple and complex position habits, visual discriminations, and delayed response: problems requiring the selective reinforcement of certain responses and the accompanying extinction of alternative responses. Such learning is thereby distinguished from that required in operating for the first time a single "work" lever, as in the aforementioned experiments.[3] With these latter problems, in contrast, the animal is presented a choice between two or more alternative responses which are almost equally preferred at the beginning of training. The response to be "learned" may even be *less* preferred, as during the early trials of a reversal of a discrimination habit.

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[2] Numerous experiments testing the effects of deprivation and reward have been carried out using the white rat as subject. Experiments on differential reward in the learning, or retention, of primates have been limited to the delayed response problem: Maslow and Groshong (1934), Nissen and Elder (1935), Cowles and Nissen (1937), Fletcher and Nissen (1937).

The first part of the investigation was therefore concerned with demonstrating whether such habits will be learned by the chimpanzee, when the incentives are food-tokens. The second part of the investigation was an attempt to evaluate quantitatively the approximate strength of the token-reward as a reinforcing agent in the acquisition of habits, and to observe any qualitative differences in behavior manifested toward this and other types of incentive. This was done by means of experimental comparisons of the acquisition of habits under the condition of food-token reward with grouped, delayed exchange of the tokens for food and under other conditions of reward, such as (*a*) direct food-reward, receiving food for a correct response without the mediation of a token exchange, (*b*) food-tokens immediately exchangeable for food after each trial, or (*c*) reward with tokens of a color different from that of the food-token, and which involved special training to teach the animal that deposit of these tokens yielded no food at all. Simple preference and work tests furnished supplementary evidence of relative incentive strengths.

II. SUBJECTS

Five young chimpanzees were used in this investigation, all of whom had been members of the New Haven Colony of these Laboratories for several years and were well adapted to experimental routines. Their names, with numerical designation, sex, dates of experimentation, and age and weight at beginning of experimentation are given in table 1.

---

[3] It is commonly observed that animals pull at objects, or operate manipulable devices, without special training or incentives.

All these animals had known the experimenter since September, 1934; and, except for Moos, had previously served as subjects for him: Alpha, Bimba, and Frank in delayed response experiments (Nissen, Carpenter, and Cowles, 1936; Cowles and Nissen, 1937), Bula in a visual discrimination experiment (McCulloch and Nissen, 1937). All had had long experience in various laboratory experiments and were being used by other experimenters contemporaneously with this investigation. Except for Frank, they also had received extensive training in the exchange of tokens for various rewards in work situations and social situations (Wolfe, 1936; Nissen and Crawford, 1936). No animal, however, had ever previously received other than direct food rewards in any of the several learning problems in which they had been subjects. Complete experimental histories of these animals are in the Laboratory files. Personality characteristics of the individuals will be mentioned when relevant. Spragg (1936, pp. 7–8) has briefly described four of the animals; the fifth, Alpha, will be treated below in some detail.

TABLE 1

NAME	NUMBER	SEX	DATES OF EXPERIMENTATION	AGE	WEIGHT
					kg.
Alpha	28	F	Jan. 1936–Feb. 1936	5 years 4 months	29.0
Bimba	26	F	Sept. 1936–Mar. 1937	7 years (?)	24.5
Bula	48	F	Dec. 1935–Jan. 1936	5 years 8 months	25.0
Frank	25	M	Jan. 1936–Mar. 1937	6 years (?)	24.0
Moos	11	M	Dec. 1935–Mar. 1937	9 years (?)	33.0

Food-hunger was kept as constant as possible by experimenting only at regular times, generally during the two and one-half hours preceding each of the two daily feedings which came at 11 a.m. and 5 p.m. It is believed that extraneous motivational influences, such as quotidian or diurnal variations or effects of contemporaneous experimentation, except as specifically noted below, did not significantly affect the results. The animals remained in excellent health throughout the experiment.

III. APPARATUS AND PROCEDURES

The apparatus was housed in two adjacent rooms on the floor below the animal quarters. The floor plan is shown in figure 1. In one room (B322) was a large, steel, *circular restraining cage* (cage A), 3½ feet high and 5 feet in diameter, in which the subject was quite free to move about. At one of two grilles, 90° apart in the cage wall, was placed a device (V 1), hereafter called a *vender*, which either automatically or by the experimenter's electric switch delivered a raisin to the animal whenever a food-token was placed in a slot, 23 inches from the floor. The food chute was 14 inches from the floor. The mechanism of this vender was similar in principle to that described by Wolfe (1936, pp. 4–5), but included an endless belt conveyer instead of the single large wheel.[4] This conveyer had over 50 separate compartments, amply large for one raisin each, 48 of which could be loaded at once. A vertically moving shutter in front of the vender made it possible for the experimenter to allow the animal access to the vender only at certain times. At the other grille could be placed, when needed, a work-apparatus (W). A perpendicular opaque *screen* (S), 4 feet square, was so placed as to conceal the experimenter, his appurtenances, and his activities.

Two work devices were used at different times. One was the *weight-lifting apparatus* described by Wolfe (1936, pp. 7–8), consisting of a lever with adjustable resistance to movement, to which could be attached a food-token. By lifting the lever upward and in toward the cage, the animal could secure the token from its original position, 24 inches from the edge of the apparatus nearest the animal. Only sufficient weight was suspended from the lever to return it to position after the animal had drawn it forward. The other work device utilized the same framework support, replacing the lever and its base with a wooden platform carrying a small *sliding tray* in a grooved runway (figure 2). By means of a light cord the animal was enabled to pull the tray within reach from its starting position 24 inches from the nearest

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[4] The writer is indebted to Mr. D. G. Ellson, Yale University, for suggestions regarding the construction of this machine.

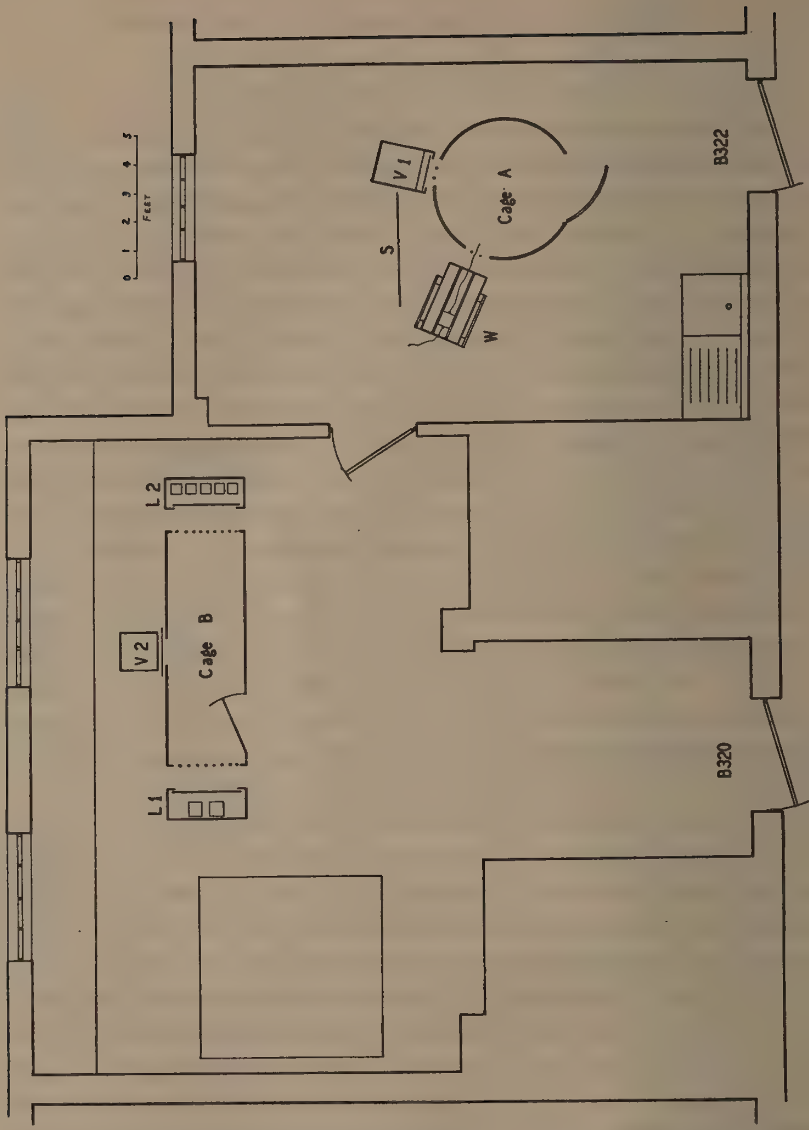


FIG. 1. FLOOR PLAN OF EXPERIMENTAL ROOMS AND APPARATUS

Cage A, work and exchange cage; W, work apparatus; V 1, vender; S, screen. Cage B, learning cage; L 1, L 2, frameworks for learning apparatus; V 2, vender.

edge of the platform. When a choice between two types of token was required, a hinged partition was so arranged in the tray compartment that if the animal moved it to one side, in order to secure either token, subsequent removal of the other token was prevented. Short, $\frac{1}{8}$-inch steel rods projected into the compartment from the midpoint of each side, preventing the token from sliding out unless the partition was displaced from its central position.

The adjacent room (B320 in figure 1) was connected to the first by an entrance closed by a heavy door, which allowed passage without using the outer public hallway. In this room was located a large, wood-frame, *rectangular restraining cage*, 8 feet by 3 feet, and 5 feet high (cage B). This was covered with $\frac{1}{2}$-inch mesh hardware cloth. At each end was a heavy grille of vertical, $\frac{1}{2}$-inch steel bars, placed at intervals allowing the animals to reach through and manipulate readily the apparatus outside the cage. Only one grille was left open at a time; the other one was closed with a removable and interchangeable wooden barrier. In front of each grille was a *permanent framework* (L 1 and L 2) bearing a learning-problem device at a distance and height convenient for the animals. An opaque drop-door was fitted vertically into the front part of the frame, so that the experimenter could close

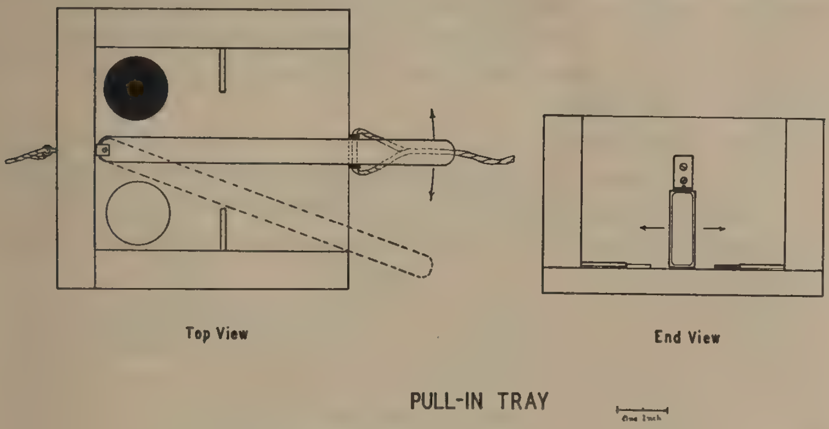


FIG. 2. PULL-IN TRAY

the device from view between trials. A shellacked Upson-board screen was fixed behind the platform to exclude the experimenter from view and to provide a homogeneous background for the problem devices. Any one of two or three different learning-problem devices could be attached to the platform, so that, with the further aid of the interchangeable barrier, the experimenter was able quickly to shift from one learning-problem to another as the order of experimental subjects required. At a side of this restraining cage, opposite to that containing the entrance, was a third, much smaller, grille, in front of which was placed a food *vender* (V 2) nearly identical to the one previously described. This also could be exposed or closed to the animal by means of an opaque drop-door controlled by the experimenter while he stood behind the wide, screened, learning-apparatus framework at either end of the cage.

General illumination of all apparatus in both rooms was provided by large electric ceiling lights, supplemented by individual lamps placed closely above the learning devices and token-slots of the venders, so as to be clearly visible both to subjects and experimenter.

The sole food used throughout the experiment was large-sized, dried, Malaga cluster raisins with seeds. These averaged about $\frac{5}{8}$-inch in diameter and numbered about 22 to the ounce. They were a food highly uniform in size and quality, and were kept refrigerated until needed. The animals always worked eagerly for them with no apparent diminution over months of experimentation. The raisins were never refused, although, when they were offered for the first time, most of the animals were slow and cautious in eating them. They quickly adapted, however, to the strange food.

What will be subsequently referred to as "tokens" were small catalin discs, $1\frac{1}{4}$ inches in diameter and $\frac{1}{12}$-inch thick.[5] These were relatively unbreakable, washable, and withstood many months of constant experimental use by animals normally quite destructive. The small size was especially advantageous when the animals were required to collect a large number and carry

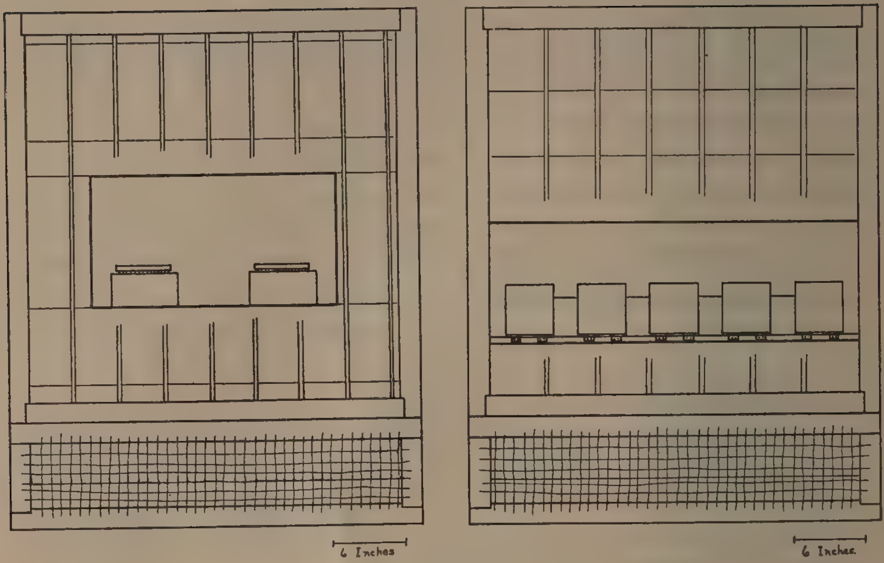
[5] Obtained from John Wanamaker, Inc., New York City.

TABLE 2

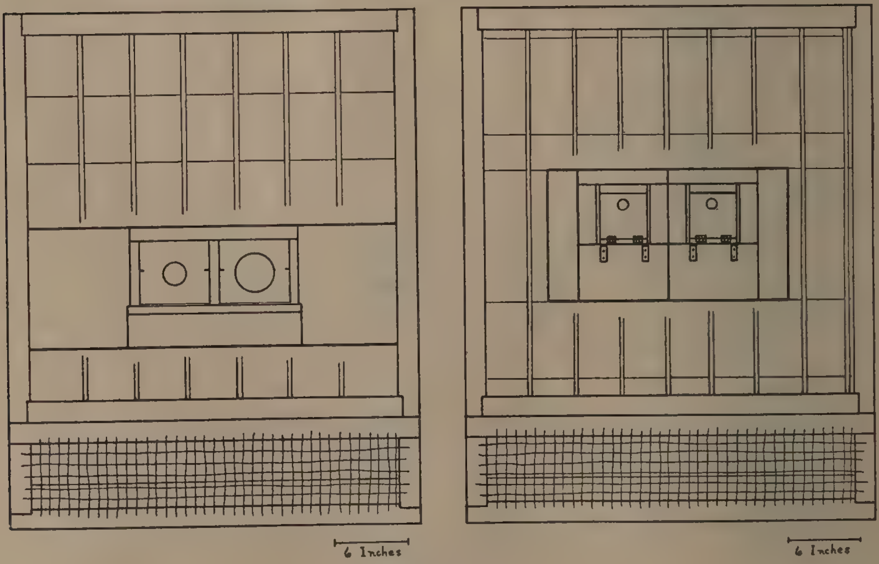
Summary of training for all animals

Description of problems, chronological sequence for each animal (numbers in parentheses), and total trials (trs.) or experimental periods (per.). FT = food-token; NFT = non-food-token; F = food; d = delayed exchange; i = immediate exchange.

PROBLEM	DESCRIPTION	FRANK	MOOS	BIMBA	BULA	ALPHA
A	Adaptation and grouped exch. FT	(1) 22 per.	(1) 12 per.	(1) 9 per.	(1) 21 per.	(1) 26 per.
B	Two-position learning, FT	(2) 460 trs.	(2) 520 trs.		(2) 140 trs.	
C	Five-position learning, FT or F					
	1. To criterion	(7) 440 trs.	(3) 360 trs.			
	2. Equal periods	(6) 400 trs.	(4) 400 trs.	(4) 400 trs. (FT only)		
D	Visual size discrimination, FT	(3) 1,120 trs.	(5) 1,240 trs.	(2) 1,320 trs.		
E	Differentiation of FT vs. NFT	(4) 24 per.	(6) 27 per.			
F	Visual color-pattern discrim. FT-only versus FT + NFT	(5) 3,132 trs.	(7) 3,168 trs.			
G	Delayed response	(8)	(8)			
	1. FTd vs. NFTd	640 trs.	640 trs.			
	2. FTi vs NFTi	560 trs.	500 trs.			
	3. F vs. NFTi	500 trs.	300 trs.			
	4. F vs. FTi	500 trs.	300 trs.	(3)		
	5. FTd only			300 trs.		
H	Preference tests	(9) 336 trs.	(9) 180 trs.	(5) 80 trs.		
	FT vs. NFT retests (probs. F and G).....	7 per.	6 per.			
	Preliminary trials (probs. B, C, D, G)...	58 trs.	58 trs.	46 trs.	12 trs.	
	Totals..............	8,146 trs. +53 per.	7,666 trs. +45 per.	2,146 trs. +9 per.	152 trs. +21 per.	0 trs. 26 per.



Two-Position Choice Five-Position Choice



Visual Discrimination Delayed Response

FIG. 3. LEARNING APPARATUS AS VIEWED BY ANIMAL SUBJECTS

them from one room to another. Although quite thin, these discs were readily picked up from the floor by the animals, or separated when adhering together.

The duration of all experimental sessions and of certain responses was timed with a split-second stopwatch.

Table 2 lists the various experimental problems set for the different subjects, with the order in which they were presented. The extent of training of each animal is indicated. Three of these experimental problems (A, E, and H) were simply work situations in which the animal was required to work for, or make a preferential discrimination between, incentives. These were carried out exclusively in the room (B322) containing cage A, the work apparatus, and a food vender (figures 1 and 2). The remaining problems were specific learning or retention problems with the usual laboratory multiple-trial procedure (figure 3) involving the repeated selective reinforcement of certain alternative responses. For the learning problems both rooms (B320 and B322) were used, unless the reward procedure called for immediate exchange. In the latter event, only cage B was employed.

A. General adaptation and training of grouped exchange of food-tokens

All the subjects were given preliminary sessions in cage A for general adaptation and to determine whether they would work for tokens exchangeable for raisins in the food vender. Except for Frank, who had had no previous experience with tokens, the animals all came to the experiment with varying amounts of training in the use of tokens and their exchange for food in a similar vender (Wolfe, 1936). For Alpha and Moos, the food-token was white; for Bula and Frank, black; and for Bimba, dark blue. Frank learned to deposit the token in one brief session of demonstration and aid by the experimenter. During the fourth experimental period the pull-in tray without partition was introduced, and from then on Frank regularly pulled in the tray, when it was baited with a token which was immediately exchangeable for a raisin. The other subjects, except Alpha, after a few trials testing their willingness to exchange for raisins the tokens handed to them, began the use of the work device during the first experimental period. Alpha's unusual timidity required a preliminary period during which no work-apparatus was present. Moos and Bula employed the weight-lifting device, with which they were already quite familiar; Alpha and Bimba began with the pull-in tray. From this point the procedure with each animal was essentially the same. As soon as the animal demonstrated that it would readily work for a food-token with immediate exchange, the collecting of two or more tokens was required before exchange was allowed. To control the opportunity for exchange, the experimenter opened or closed the shutter in front of the vender. The number of tokens to be worked for before exchange was gradually increased as the willingness of the animal permitted. Verbal encouragement was often employed to bring the animal back to work after unusually long pauses occasioned by play activity, responses to outside distracting sounds, or other irrelevant behavior. The aim was to establish a practical limit in excess of the number of tokens later to be received in a different (learning) situation. The limiting size of the group of tokens to be worked for before exchanging was arbitrarily determined by the rate at which the animal would work: repeated pauses of more than half a minute, even with verbal encouragement from the experimenter, indicated this limit. When the animal was working quite readily and consistently for such a group of tokens, it was given a session or two of adaptation to the other room and cage B by being taken there and handed a few tokens for later exchange in cage A. With the succeeding experimental period, regular trials were begun in cage B, using learning apparatus.

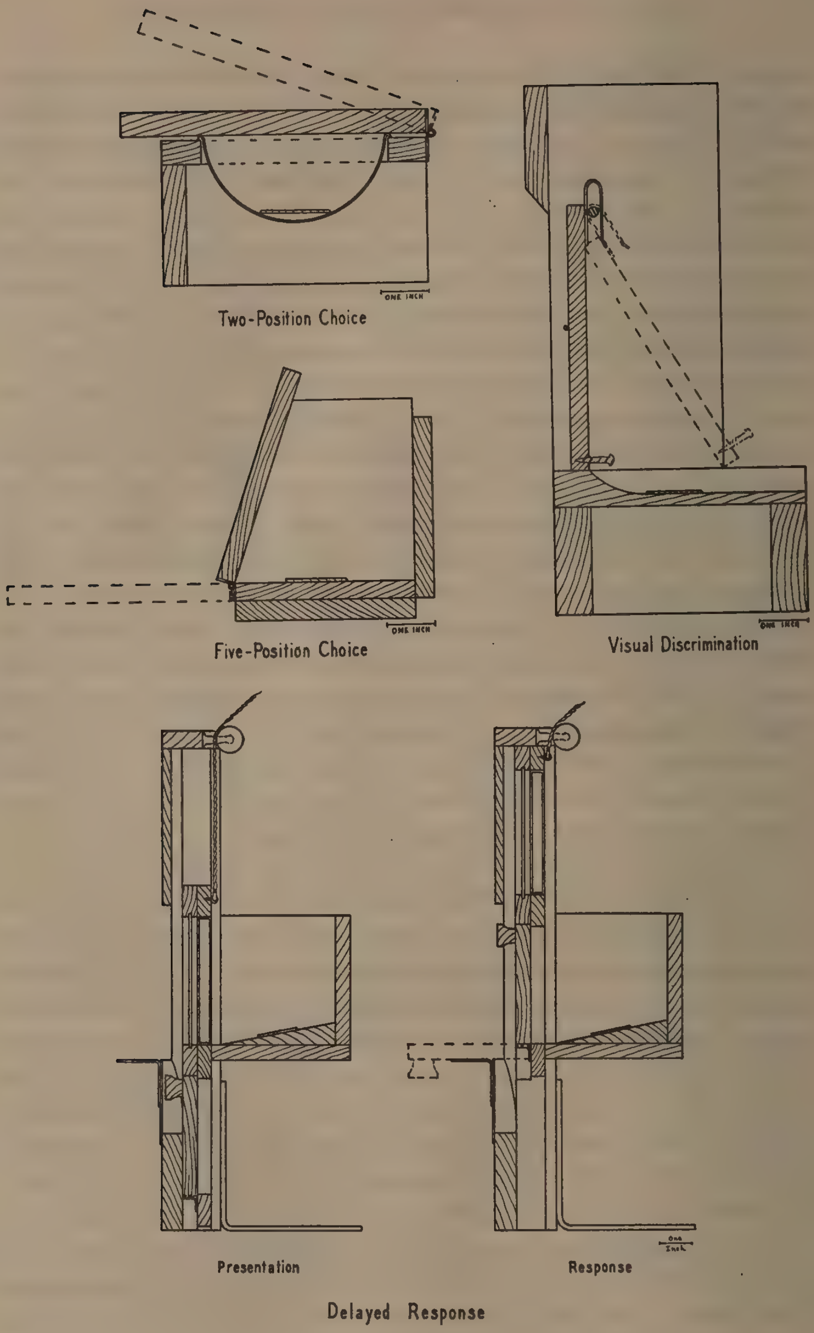
General procedure for B, C, and D. At each learning session the animal was first taken to cage B and there given a series of from 10 to 36 trials without interruption. The number of trials was generally fixed at 20 per period. For each response to a particular ("positive") stimulus the animal received a food-token. At the completion of the group of trials, the animal, carrying the tokens in its hand or mouth, was led by the experimenter to the next room and was allowed, about 50 seconds after the last learning trial, to exchange each of the tokens for a single raisin in the cage A vender. The animal was required to walk a distance of approximately 35 feet from the center of one cage to the other. The animal was then taken from this exchange cage and returned to the living cages on the upper floor. In the case of the animals whose work limit was a group of 20 or less tokens before exchange, the number of learning trials was first set at 10 per session, but with two successive sessions given in any experimental period. The number for each animal is given under the specific procedures below. As training progressed, it was found that the number of trials in each of these two sessions could be increased from 10 to 20. The animal returned to cage B for the second set of trials after exchanging in cage A the tokens received during the first session. The animal was then taken to the living quarters after exchanging the tokens of the second session.

These three learning problems (B, C, and D) were presented as outlined below. Certain preliminary trials for each were introduced, as will be described, which served to acquaint the animal with the manipulation of the apparatus and the routine of the experimental periods to follow. Since all stimuli used were equally rewarded for these trials, they are accordingly to be considered work- rather than learning-trials.

B. Two-position learning, food-token incentives only

The two-position learning apparatus (figure 3, upper left) consisted principally of two aluminum cups, each within a wooden box closed by a horizontal lid, hinged at the rear. The animal had merely to lift the projecting front edge of the lid to reach the contents of the cup. The boxes were finished in natural wood color. This learning apparatus was regularly located in learning-apparatus framework L 1 (figure 1); the distance from center of cups to the cage grille was 15½ inches (see also figure 4 for details of construction of learning device).

In this problem a particular one of the two closed containers (for example, the right-hand one) was baited with a food-token in successive trials until the animal met a criterion of choosing a



Two-Position Choice

Five-Position Choice

Visual Discrimination

Presentation

Response

Delayed Response

FIG. 4. CROSS SECTION DETAIL OF LEARNING DEVICES

container consistently upon the first choice of each trial. The other box was then baited regularly until the animal changed its previously learned response and met the same criterion of choice of the other container. Such "reversals" of habit were then repeated.

A trial consisted of these operations: the drop-door was first opened by the experimenter, and a container was selected by the animal, who removed the token if it was found there; if unsuccessful, he was allowed immediately to open the second container and remove the token; the drop-door was then closed by the experimenter. Between such trials the experimenter opened both boxes, baited one as quietly as possible, and closed both noisily as a ready signal to the animal.[6] Ten seconds after the drop-door was last closed, it was reopened for the next trial.

Each subject first received 12 preliminary trials for general adaptation to the apparatus; each container was baited an equal number of times with a food-token, in an irregular, balanced order. A second choice was allowed in case of error.

In the regular learning trials which followed, the criterion of mastery was adjusted to the ability of the subjects; for Moos, the same container was to be chosen for 90 per cent or more trials in two consecutive 20-trial periods; for Frank, this was lowered to 80 per cent or more in two consecutive 20-trial periods; and for Bula, the criterion was still lower, being 80 per cent or more in only one 20-trial period. Bula and Frank worked for two 10-trial sessions in each experimental period, being allowed to exchange the tokens for food after each set of 10 trials; Moos was required to work for one continuous 20-trial session before exchanging. The interval between reversals was from 6 to 18 hours (the next possible experimental period).

C. *Five-position learning, food-tokens or food*

The five-position learning apparatus (figure 3, upper right) consisted of five identical wooden boxes arranged side by side.

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[6] Frequent control against auditory cues was exercised: both boxes were opened while a token was rattled noisily in each, simultaneously or successively. The activity and noise of the animal between trials indicated that he was not attempting to utilize such cues.

The front faces sloped slightly backward and were hinged at the lower front edge of the boxes, so that the animal, in order to secure the contents, had only to swing forward and downward the projecting upper edge and to slide the object toward himself along the back side of the cover. While the boxes were open at the top to facilitate baiting, the interiors were at no point visible to the animal. The inside surfaces were painted with white enamel, the covers with gray enamel. When in use, this apparatus was always placed in learning-apparatus framework L 2 (figure 1) with a distance of 20 inches from the center of the boxes to the cage grille (see figure 4 for details of construction of learning device).

In this problem the procedure for each trial was essentially the same as in the previous problem. When the drop-door was raised, successive choices of the different boxes were allowed until the animal found and removed the incentive. Frank and Moos each received 10 preliminary trials for general adaptation to the apparatus. Bimba received none. During these preliminary trials, each of the five boxes was baited with a food-token in successive trials, the positions being presented twice each and in an irregular order. Regular learning trials followed in the next experimental period.

An important difference between this and the preceding two-position problem, aside from the greater complexity due to the greater number of choice points, lay in the manner of baiting. A single box was loaded at any given trial. Series of trials were given in which each successful response to the predetermined container secured a food-token, exchangeable in cage A only after each 20-trial period; other series of trials were given, each of which was rewarded by the food directly (one raisin in the baited, or "correct," container). Two methods were used for limiting the series of trials with the same type of incentive.

One method was a "criterion technique" whereby the five position-habits were learned successively, each to a definite criterion of consecutively correct choice. As soon as response to one box was established, a different box was baited until the animal chose it consistently to the same criterion. The interval between habits was from 6 to 18 hours (next possible experimental period). The five habits and their incentives (raisins or tokens) took the following order (numbering the boxes from the experimenter's left): no. 2—food, no. 4—token, no. 1—food, no. 5—token, no. 3—food, no. 2—token, no. 4—food, no. 1—token, no. 5—food, no. 3—token. Each habit was thereby learned once with each type of incentive. The criterion of mastery was first choice of the baited container for 80 per cent of the trials in a single 20-trial period.

The other method of marking off the series of trials was an "equal periods technique," whereby equal groups of 20 trials were devoted in irregular order to the five different positions, instead of continuing until a criterion of mastery be met. One type of incentive (raisin directly or token exchangeable after the group of trials) was used throughout each 20-trial period. Two 20-trial periods were devoted to each pair of conditions, position and incentive, arranged in such an order as to balance the periods between morning and afternoon.

Order of habits and incentives

DAY	1	2	3	4	5	6	7	8	9	10
a.m.	F–2	T–1	F–3	T–4	F–5	T–2	F–1	T–3	F–4	T–5
p.m.	T–4	F–5	T–2	F–1	T–3	F–4	T–5	F–2	T–1	F–3

F = food; T = food-token; numbers represent positions of baited containers from experimenter's left to right.

Each position was therefore baited for 80 trials, two periods of 20 trials each for each type of incentive, one during morning and one during afternoon. With one subject, Bimba, these five positions were baited with food-tokens only; the order and number of trials each followed the above table, substituting T for F wherever the latter appears.

D. *Visual size discrimination, food-tokens only*

The visual discrimination apparatus (figure 3, lower left) consisted of a small framework from which were suspended vertically two wooden stimulus plates, hinged at the top edges. Behind and below each plate was a small depression in the platform, in which could be placed the reward. When pushed at the lower edge, the plate swung inward, exposing the reward.

As in the previously described devices, the reward was visible only after choice had been made by opening the proper compartment. The stimulus plates were interchangeable, permitting not only a rapid shift in position of the pair being employed, but also permitting ready change to another pair of stimuli. These plates were $5\frac{1}{2}$ inches square and $\frac{1}{2}$ inch thick, and were painted with white enamel; on the face of these were then drawn or painted the stimulus pattern, the whole being finally coated with transparent shellac. They could then be subjected to frequent cleaning. This apparatus was regularly placed in learning-apparatus framework L 2 (figure 1), with the center of the reward compartments 17 inches from the cage grille (see figure 4 for details of construction of learning device).

In this problem a single outline of a circle was described with India ink on each of the two stimulus plates. One had a radius of 0.96 inch ($\sqrt{6}$ cm.), and the other had a radius of 1.67 inches ($\sqrt{18}$ cm.), making enclosed areas in the ratio of 1 to 3. The width of the outlines was $\frac{1}{20}$ inch; the circles were centered on the stimulus plates. Both circles were simultaneously presented in each trial, and response to a particular size of circle regularly secured a food-token, although this circle's position was shifted from right to left in an irregular balanced order.

Frank and Moos each received 20 preliminary trials for general adaptation to the apparatus, during which the two stimuli were *both* baited with food-tokens, only one choice being allowed per trial. Neither animal showed any preference for either stimulus, and the choices resulted in equal rewarding of both stimuli. In the case of Bimba's 30 preliminary trials, the differential learning stimuli were eliminated through the use of an unmarked stimulus plate in a single unit of the discrimination apparatus. In the regular learning trials four orders of presentation were used in rotation (A, B, C, D, A, B, C, D, and so forth), the position (for the animal) of the baited stimulus being indicated:

A. LRRLRLRLLLRRLRLRRRLL
B. RLLRLRLRRRLLRLRLLLRR
C. LLRRRLRLRRLLLRLRLRRL
D. RRLLLRLRLLRRRLRLRLLR

At each trial, the drop-door was opened and the animal made its choice. If it failed to choose the baited stimulus first, a second choice was allowed, but only after the drop-door was closed and reopened. The experimenter meanwhile did not replace, before second choice, the stimulus plate previously moved. Two 10-trial sessions were given Frank and Bimba in a single experimental period, with the tokens exchangeable for food in cage A only after each of the two sessions.[7] Moos was given 20 uninterrupted trials per period, with exchange allowed in cage A only at the end of the period. The criterion of mastery for all animals was the selection of the same sized circle for at least 85 per cent of the trials in the last 40 trials. The other circle was then regularly baited until the animal met the same criterion of response to this circle as for that previously learned. The interval between habits was 6 hours for Moos, 24 hours for Frank and Bimba.

E. Training of preferential differentiation of food-tokens and non-food-tokens

In each of the previous problems, the animals had had experience with only one type of token, one exchangeable for a raisin in the cage A vender. In the present part of the investigation, Frank and Moos received thorough training in the use of a token which, when deposited in the vender, would deliver no food at all.[8] This was accomplished by employing the pull-in tray (figure 2) with or without the swinging partition, as well as delivering the tokens to the animal by hand, or presenting them on the edge of the runway of the tray. The color of the non-food-token was selected to be radically different from that of the food-token: white for Frank, and blue for Moos. The assumption that these animals could easily discriminate from the outset the brightnesses of the two types of tokens they were to use was verified by the outcome of their training.

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[7] The number of trials in a session before exchange was extended in Bimba's case, after the first 12 sessions, from 10 to 12 trials.

[8] A difference in sound made by the vender with each type of token was occasioned by the fact that the food-delivering mechanism did not operate when the non-food-token was inserted. Control tests, in which food was released for a food-token but the mechanism did not operate, seemed to indicate that the animals were not responding to such a difference.

The following four types of training were carried out until the animal demonstrated unerring choice of the food-token in preference to the non-food-token (during type 4): (1) successive single non-food-tokens (*or* food-tokens) were presented by hand or by pull-in tray for immediate exchange (vender open during work); (2) a single type of token was presented in successive single pull-ins for later grouped exchange (vender closed during work); (3) pairs of food-token and non-food-token were presented for immediate exchange of the chosen token (vender open during choice); (4) similar pairs of food-token plus non-food-token were presented for successive choice, but with delayed grouped exchange of the chosen tokens (vender closed during work). In all cases the positions of the tokens were shifted according to the irregular balanced orders listed above for problem D (p. 20). Each of the four methods of presentation, (1) to (4), was given not only as pull-ins and with removal of tokens from the tray with partition, but also as free choices from the tray without partition. In the latter case, the animal was free to remove both tokens together, as was not easily possible with the partitioned tray. Since it was established during training that the least consistent choice of the food-token in preference to the non-food-token occurred under condition (4) above, especially when the pull-in tray was employed, the criterion of choice was fixed at two successive groups of 18 paired choices each, using the pull-in tray with vender closed, and exchange allowed only after each group of 18 trials.

General procedure for F and G. The following problems were employed not only as means of investigating the reinforcing strength of the food-token in further typical learning problems, but also to compare its reinforcing strength with that of food received directly (as in problem C above) and with that of the token which yielded no food upon deposit in the vender.

The precise exchange procedure for the non-food-tokens, under the usual grouped exchange conditions, depended upon the behavior adopted by the animal. In general, the non-food-tokens and food-tokens alike were always removed from the learning apparatus by the animal. The non-food-tokens, however, were usually left in cage B when the food-tokens were removed by the animal for exchange in cage A. If any non-food-tokens were carried to cage A, they were deposited either after the food-tokens had been exchanged, or not at all. No attempt was made to force the animal into a particular mode of response with reference to the non-food-tokens, except as noted below for immediate exchange. Variations often occurred within the same animal's work on the same problem, which were beyond the experimenter's control; in any case, great care was taken not to deliver food to the animal if a non-food-token was deposited; the experimenter watched the color of the token and controlled the vender with the key switch. The animals regularly showed characteristic differences in behavior toward each of these types of tokens at the time of exchange, which indicated continued differentiation even when the non-food-tokens were not treated differently by them at the time of the learning trials. Any lapse of differentiation could only result in further differential training of the non-food-token, by failure to secure food at exchange.

There were times when but one type of token had been used for many trials and the procedure then called for use of both types of tokens during subsequent trials. In order to verify the animal's ability to differentiate between food-tokens and non-food-tokens, and since the first session of trials with the later procedure would precede any differential reinforcement of the tokens by exchange, separate special preference tests were first given in cage A with the pull-in tray. These tests were given as an additional period on the same day as the regular experimentation periods, either in the late forenoon or afternoon, so as to preserve the maximal interval of time from any adjacent experimental period.[9] The procedure in these retest periods was simply to present two sets of 18 paired, pull-in choices of food-token versus non-food-token, with exchange allowed only after each group of 18 trials. This was the same pattern of procedure as the final periods of differentiation training under problem E above. The occasions and results for these retest periods will be discussed below.

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[9] Not less than four hours from the following period, or two hours from the preceding period.

A further new condition was introduced in problem G with the immediate exchange of tokens. In this case a slight change in the handling of the animals was necessary: instead of being removed from the learning cage B to vender 1 in the adjoining room, they were allowed to exchange the tokens in vender 2 immediately after each trial without leaving cage B. At the conclusion of the experimental period, the animal was taken directly from cage B to its living cage. In order to render the trial to trial procedure as uniform as possible for each type of token during the immediate exchange technique of problem G, it was found necessary to train the animal to deposit the non-food-tokens in the vender rather than to leave them in the cage during succeeding trials. This eliminated the possibility of the animal's attempting to deposit the non-food-token only at the time of exchange of the food-token. It also prevented the accumulation of non-food-tokens while the food-tokens remained immediately exchangeable.

F. Visual color-pattern discrimination, food-tokens only, or food-tokens and non-food-tokens

This problem (figure 3, lower left, except with different stimulus patterns) utilized the same mechanical device and location as did the visual size discrimination problem D (see also figure 4). Different stimulus plates were used however: one rectangle measuring 1.97 inches by 3.94 inches (5 cm. by 10 cm.), painted with cardinal red lacquer on a white enameled background, was centered vertically on one plate; another rectangle of the same size, painted with dark green lacquer on white enameled background, was centered horizontally on the other plate. No preliminary trials were given. Both figures were simultaneously presented in each trial, and no second choice was allowed as in problem D. A single 36-trial session was given to Moos in each experimental period before exchange of tokens was allowed in cage A. Two 18-trial sessions were given to Frank in an experimental period, with exchange after each series of 18 trials.

A particular method of baiting was carried out with respect to each stimulus plate, although its position was shifted from left to right in an irregular balanced order, until the animal chose one plate to a certain criterion. This criterion was: at least 12 choices of the same stimulus in one-half period of 18 trials, followed by at least 17 choices of that stimulus in the succeeding half-period of 18 trials. These consecutive half-periods need not have been in the same experimental period. When a habit was thus learned, response to the other stimulus plate was trained to the same criterion, not only by shifting the stimulus position of the food-token, but, in certain cases, by using different incentives. Each reversal of habit was begun at the next possible experimental period (from 6 to 48 hours' interval). Two methods of baiting were employed: (1) food-token with one stimulus plate, *no token* with the other; (2) food-token with one stimulus plate, *non-food-token* with the other. Since there were two stimuli (red or green), either of which might be "positive" and with two methods of baiting, (1) or (2), orders of the four conditions of learning were arranged to permit alternate reversals of the discrimination habits but with a mixed, balanced order of baiting methods. The following order of habits was used: A2, B2, A1, B1, A2, B1, A2, B1, A1, B2, A2, B2, A2, B1, A1, B2, A1, B1, A1, B1, A2, B2, A1, B2, A2, B2, where (A) represents one stimulus plate and (B) represents the other, while (1) and (2) refer to the above-mentioned baiting conditions. The letter (A), or (B), indicates the position of the food-token in each trial. In any experimental period, the order of position (for the animal) of the stimulus baited with food-token was either:

I. RLLRRLRLLRLRRRLLRL RLRRLLLRLRRLRLLRRL

or,

II. LRRLLRLRRLRLLLRRLR LRLLRRRLRLLRLRRLLR

These orders were used alternately in successive experimental periods, except at the beginning of a reversal of habit; thus an alternation in the above orders of presentation was obtained for the food-token itself.

The retest periods described above were given whenever a habit rewarded by method (1) was followed by any habit rewarded by method (2). There are six such occasions in the complete order of habits given above.

G. Spatial delayed response, differential accuracy for food-tokens, non-food-tokens, or food; immediate or delayed exchange of tokens

The delayed response apparatus (figure 3, lower right) consisted principally of two raised boxes, open at top and front, and fixed side by side, each behind a small frame fitted with a vertical shutter. The upper half of each shutter was a glass window the size of the open front of the box. When lowered in place before the box, the window gave view to the interior and its contents. The lower half of each shutter was a small wooden door which, when exposed by raising the shutter, could be opened forward until it rested on angular supports and became level with the floor of the box behind it. The contents could then be easily removed. A pulley system with counterweights enabled the experimenter to drop both shutters simultaneously, thus exposing the box interiors through the glass windows; or to raise the shutters, thereby closing the boxes from view and leaving the doors accessible to choice and opening by the animal. Lowering either shutter automatically closed the door if previously opened. Adequate shadowless illumination, in addition to a 13° inclined floor in each box, insured clear visibility of the incentive. The frames of the apparatus were painted dull black, but the small wooden doors and interiors of the boxes were shellacked a natural wood color. This whole apparatus was regularly located in learning-apparatus framework L 1 (figure 1); the distance from center of boxes to the cage grille was 17½ inches (see figure 4 for details of construction of this device).

The following procedure was maintained throughout all delayed response trials. The drop-door was raised (the animal's "ready" signal), followed by simultaneous demonstration of the contents of both boxes by dropping into position the window halves of the small shutters. Only one box had previously been baited with a single incentive placed in the center of the inclined floor.

After exposing the interiors for fully three seconds, the boxes were closed by raising the small shutters, and the drop-door was lowered. The delay period began with the closing of the small shutter doors of the boxes. After the delay interval, timed by stop-watch, the drop-door was raised and the animal was allowed to open one box. If this were the baited box, the animal removed the food or token. If the unbaited box were chosen, the drop-door was immediately lowered for one second and raised again, the previously chosen box being left open; the animal was then allowed to open the baited box. Each trial was therefore concluded with the animal obtaining the previously demonstrated incentive. During a 20-second interval before the following trial, the experimenter quickly baited one of the boxes and closed the small box doors. The next trial was then conducted in the same manner.

Before any regular delay-trials were given, 16 preliminary trials with minimal delay, in which both boxes were baited an equal number of times with a food-token, were given to each animal to adapt it to the manipulations and routine involved.

With Frank and Moos as subjects, the following pairs of incentives were compared in separate series of from 300 to 640 trials each, in which each of the two incentives was presented an equal number of times in a mixed order. The interval between series was from four to six days.

1. Food-token *vs.* non-food-token, delayed exchange in cage A
2. Food-token *vs.* non-food-token, immediate exchange in cage B
3. Food immediate *vs.* non-food-token, immediate deposit in cage B
4. Food immediate *vs.* food-token, immediate exchange in cage B

Each experimental period invariably consisted of 20 trials, 10 with each type of incentive. In the case of delayed exchange, the tokens were allowed to be exchanged in the adjoining room only at the end of the entire experimental period of 20 trials. The succession of incentives and their positions (for the animal) followed these orders, which were used in rotation:

A. L− R− R+ L+ R− L+ R+ L− L+ L− R+ R− L+ R+ L− R− R− R+ L− L+
B. R+ L+ L− R− L+ R− L− R+ R− R+ L− L+ R− L− R+ L+ L+ L− R+ R−
C. L+ L− R+ R− R− L− R+ L+ R− R+ L− L+ L− R+ L+ R− L+ R+ R− L−
D. R− R+ L− L+ L+ R+ L− R− L+ L− R+ R− R+ L− R− L+ R− L− L+ R+

The (+) represents the food-token when being used concurrently with the non-food-token, and also represents the food (raisin) when being used concurrently with either type of token. The (–) represents the other incentive being compared in each case.

Bimba was given delayed response trials with the food-token only, delayed exchange in cage A, as incentive. Each experimental period consisted of two 20-trial sessions with token exchange allowed after each series of 20 trials. The positions of the food-token followed the above orders A, B, C, D, in rotation, the food-token being incentive in the case of both (+) and (–).

H. Preference tests for all incentives

These tests were all conducted in cage A and employed the pull-in tray (figure 2), upon which were paired at each trial the two incentives being compared. The animal was allowed only one choice from any pair; the swinging partition facilitated this requirement. In case of delayed exchange, the vender door was closed during each group of 20 choices; exchange was allowed at the completion of the twentieth choice by opening the vender door. The following pairs were presented in continuous series of 20 to 40 trials each, the number being fixed for each animal. The numbered order also represents the sequence of the series.

1. Immediate food *vs.* non-food-token, delayed deposit
2. Immediate food *vs.* food-token, delayed exchange
3. Immediate food *vs.* non-food-token, immediate deposit
4. Immediate food *vs.* food-token, immediate exchange
5. Food-token *vs.* non-food-token, delayed exchange
6. Food-token *vs.* non-food-token, immediate exchange

The position of the first named incentive of each of the above pairs was determined by the orders A, B, C, and D (see problem D, p. 20) used in rotation. Following these series of choices, groups of mixed pairs were given, presenting each of the three pairs with delayed exchange (pairs 1, 2, and 5 above) in a mixed balanced order for 24 to 48 choices. Groups of mixed pairs with immediate exchange (3, 4, and 6 above) were then presented. The particular pair used and the position (for the animal) in the tray, of the first named incentives, followed this order:

1L 2R 5R 2L 1R 5L 5R 1L 2L 5L 2R 1R
5L 1L 5R 2L 1R 2R 2L 1R 2R 5L 1L 5R

The reverse order was also given for a 48-trial period. In the case of Frank or Moos the numbers 1, 2, and 5 were substituted respectively with 3, 4, and 6 to form the other series of pairs. Bimba was given continuous series of pairs, types only 2 and 4, without any mixed series.

IV. RESULTS

All results will be presented according to the sequence of problems given in the first column of table 2 (p. 11). This is not the exact order in which these problems were given to each animal, but it will facilitate comparisons and conclusions with regard to the specific experiments or individual performances.

A. General adaptation and training of grouped exchange of food-tokens

Prerequisite to all the learning problems to follow was the preliminary training given to each animal in cage A (figure 1). This was to accomplish three aims: to adapt the animal to the experimental situation and routine; to train it in the manipulation and exchange of the tokens for food; and, most important of all, to train it in working for whole groups of tokens before exchange for food.

The adaptation and training are summarized for each animal in table 3. It will be seen that the number of experimental periods necessary for accomplishing our purposes varied; this depended on the previous experience of each subject and the difficulty with which a reasonably large maximum group of tokens was attained. The number of sessions devoted to the specifically work situations in this preliminary training is divided roughly into halves, so that increases in the number of tokens per period may be noted, as well as the increase both in the maximum in any group and in the mean of maximums for different sessions. The mean time per token, worked for and exchanged, is a rough index of the rate at which the animals worked, and is seen to be on the increase from the first half of the periods to the second half of the periods,

TABLE 3

Summary of preliminary training

General adaptation and training to work for and exchange groups of food-tokens. For each subject is given: periods, number of tokens, and time.

TYPE OF TRAINING	EXPERIMENTAL PERIODS	NUMBER OF FOOD-TOKENS				MEAN TIME PER TOKEN WORKED FOR AND EXCHANGED
		Total received and exchanged	Mean number per period	Maximum in any group	Mean maximum	
Alpha						
						seconds
Cage A adaptation	1	10		2		
Pull-in	2–7	125	21	13	5	35.1
Pull-in	8–13	133	22	13	9	27.9
	2–13	258	22	13	7	31.6
Cage A adaptation*	14–26	20		5		
Bimba						
Pull-in	1–4	51	13	6	4	30.0
Pull-in	5–8	45	11	10	9	43.0
	1–8	96	12	10	6	35.7
Cage B adaptation	8(a)	10		7		
	9	20		10		
Bula						
Weight lift	1–10	184	18	8	4	42.1
Weight lift and pull-in	11–20	246	25	15	10	33.6
	1–20	430	22	15	7	37.3
Cage B adaptation	20(a)	5		3		
	21	31		10		
Frank						
Operation of vender	1–3	50		6		
Pull-in	4–12	182	20	12	8	33.8
Pull-in	13–21	260	29	20	16	26.3
	4–21	442	25	20	12	29.2
Cage B adaptation	22	10		5		

TABLE 3—*Concluded*

TYPE OF TRAINING	EXPERIMENTAL PERIODS	NUMBER OF FOOD-TOKENS				MEAN TIME PER TOKEN WORKED FOR AND EXCHANGED
		Total received and exchanged	Mean number per period	Maximum in any group	Mean maximum	
			Moos			
						seconds
Weight lift	1–6	211	35	31	16	33.1
Weight lift	7–12	240	40	30	30	20.6
	1–12	451	38	31	23	26.4
Cage B adaptation	12(a)	2		2		

* These additional periods were interspersed among periods 2–13; hence, in the case of this one entry, the numbers 14–26 do not indicate exact chronological order.

(a) indicates this session was part of the experimental period whose number is given above.

except for Bimba. This animal, while always remaining eager to pull in tokens when encouraged by the experimenter, tended in the latter part of training to become slower in her response to offers of tokens. As subsequent excellence of motivation during the long sessions of learning trials to follow demonstrated, this slowing in rate may have been due to a growing "psychic satiation" with the problemless, repetitive work situation, rather than to any diminution in the incentive value of the tokens.

For two of the five animals, this preliminary training involved difficulties that were seemingly beyond the experimenter's control. Alpha developed a particularly strange fear of the situation after four successful sessions, and began to refuse with increasing persistence to enter the work cage when brought to the experimental room. The source of this fear was not determined; it did not seem to be correlated with any unusual sights or sounds in the room, with the food, or with the tokens, nor caused by the experimenter's presence. Relations with the latter had always been entirely friendly. When finally coaxed into the work cage, Alpha did upon occasion show a willingness to work for tokens, even to the extent of working for as many as ten or more before exchanging them, something she had not been trained to do while a subject for Wolfe (1936). In his experiment, as in this one, Alpha required more sessions for general adaptation than any of the other animals. Alpha was finally dropped as a subject for the further problems; her recalcitrance and distractibility, however little they seemed to be the result of the special conditions of work-for-tokens, would have rendered subsequent training difficult and of doubtful outcome.

Bula's difficulty was one of marked uneasiness in the work cage, very likely because at that time she lacked her usual chimpanzee companions. She was a subject also in another investigation (Crawford, 1937) in which she had practically constant companionship, particularly with her close friend Bimba. It was often difficult to separate Bula from Bimba, in removing the former from the living cage; and when in the experimental room, Bula went through the necessary experimentation with impatience and frequent attitude of compulsion "for the experimenter's sake." She always left the situation with much alacrity. This behavior became more pronounced as time passed, and it was found advisable not to compel her to continue further than her successful learning of the two-position choice, problem B, to be described. In the work situation in cage A, she proved to be a less willing worker than the other animals, except Alpha. Only with persistent verbal encouragement would Bula continue pulling in tokens beyond the first two or three. Her previous experience had included considerable training in only the immediate exchange of food-tokens (Wolfe, 1936), and this with her general impatience to leave the situation may have limited the maximum number of tokens she would collect before exchange. After twenty sessions, it was concluded that further work-training would not materially increase the number of tokens for which she would work, and preliminary adaptation to cage B was given by leading her there from cage A and giving her several tokens for subsequent exchange in cage A. Initial reluctance to go from cage A to cage B decreased with repetition during the following session. Preliminary trials in cage B with the two-position learning apparatus were begun the following experimental period.

Bimba, Frank, and Moos each readily adapted to the experimental situation and invariably displayed complete ease and confidence. Frank, to whom tokens and their exchange were a new experience, very quickly learned the operation of the vender and soon substituted skillful movements for his early awkward manipulations in exchanging the tokens. (This involved a shift of apparent temporary left-handedness to his more usual right-handedness.) Like Moos and Bimba, both of whom had had previous experience in the working for groups of tokens for exchange (Wolfe, 1936), Frank eagerly pulled in the tokens one after the other with very little verbal encouragement from the experimenter. All three animals, unlike Alpha and Bula, frequently vocalized their general pleasure. With none of them was it felt that the tabulated "maximum number of tokens collected" in any group really represented the maximum obtainable with further training. It was only expected to accustom the animal to collecting fairly regularly a group somewhat larger than the expected number of trials in the learning sessions to come. In the case of Bimba, the work-training was actually shortened in the hope, later realized, that the animal would perform an increasing number of learning trials before exchange, as experimental periods continued.

Each of these three animals received preliminary adaptation to cage B before beginning trials with the specific learning apparatus therein, much the same as has been described for Bula. Since Bimba was to begin with problem D, a two-stimulus visual discrimination problem, a device was arranged to give her some preliminary practice in manipulating the learning apparatus without contact with the stimuli to be discriminated later. This was simply a single unit of the same dimensions as one of the choice compartments in the regular double apparatus; the stimulus plate in this single unit was painted white and left blank. Bula, Frank, and Moos began their learning problems with problem B, the two-position problem. In their case, a standard series of random rewarding of the two boxes was used as preliminary training in the manipulation of the containers. In these preliminary trials with the learning devices the animals behaved quite as they have been observed to do at the outset of similar problems where food is a direct reward: almost breathless eagerness to operate the devices, handling them carefully, and quickly perfecting the necessary movements. Removing the tokens from the apparatus presented no difficulties whatever to the animals.

A few words should be said here regarding the manner in which the animals handled the food-tokens, characteristics persisting with little change throughout experimentation with all the animals. Tokens removed from the learning apparatus were generally held in the mouth and less frequently dropped on the floor of the restraining cage. In removing them from cage B for exchange in cage A, the animal always picked up whatever tokens happened to be on the floor as soon as the experimenter went to the cage door to lead the animal out. All the tokens were carried in the mouth during the walk from the former cage to the latter. Frank was the only animal to carry tokens in his hand from one room to the other. He did this for about a month early in training, but he voluntarily abandoned it in favor of mouth-carrying. The tokens were removed from the mouth in cage A and carefully separated and exchanged singly. It was a usual feature of exchanging for the animal to precede each insertion of a token into the vender by first putting it into the mouth, as if to facilitate grasping the token at one edge, and then while holding it in line with the token-slot to deposit it.

It might also be said, in reference to any intrinsic value of the tokens to the animals, that the animals did not regularly play with the tokens they had collected either during, between, or after the learning trials. Much, and frequently noisy, mouthing of the tokens did occur, as the animal held them in the mouth; but this activity seemed to accompany more or less profuse salivation at the presence of the tokens, rather than to represent any play activity. Play activities not involving tokens did occur in considerable variety, often persisting as stereotyped patterns of behavior over many days of experimentation. Some of these will be mentioned below when relevant to other results.

B. Two-position learning, food-token incentives only

The purpose of this problem was to present the animals with as simple a learning problem as could be set, and yet one which might require more than a single trial for mastery (figure 3, upper left). If food-tokens, as trained above, are but very weak incentives in a situation so removed as this in time and space from the point of exchange, then such a simple learning problem would give favorable opportunity for demonstrating the reinforcing strength of these tokens. Reversals of the habit, according to the position of the token-incentives, may be regarded as unmistakable evidences of learning, since a single habit might be established by simple position preference from previous experiences (or attempted solution in case of failure) and hence be quite independent of the method of rewarding. Repeated reversals elimi-

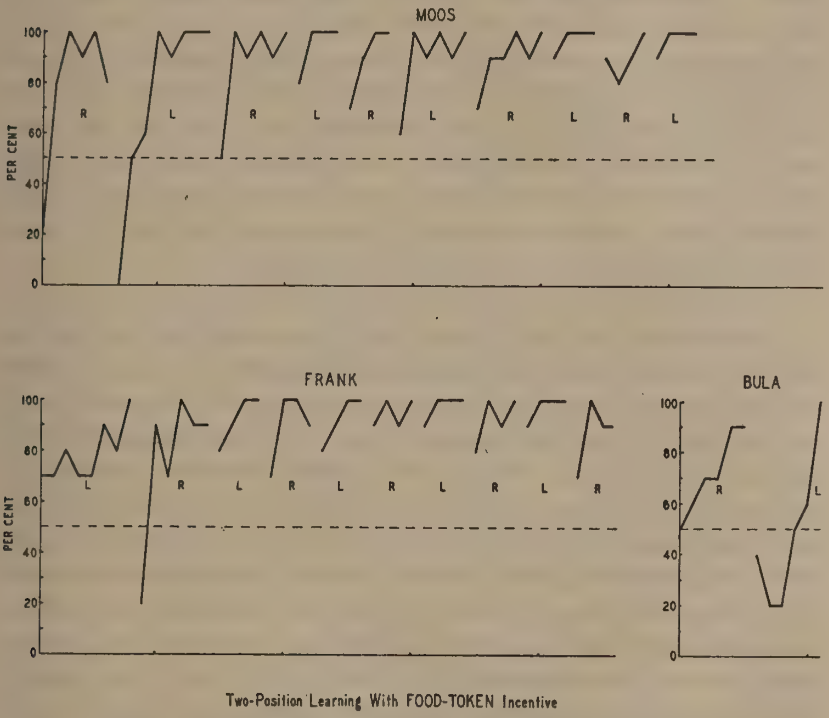

Fig. 5. Learning Curves for Two-Position Problem

Each point represents per cent choice of food-token baited container in ten trials.

nate the possibility of preferences coincident with, but not related to, the experimental changes.

With these considerations we may consider the data presented by the graphs of figure 5 as indubitable evidence that three animals learned certain right- and left-position habits in which the essential differential reinforcement was provided by these food-tokens, exchangeable after 10 or 20 trials as previously described. The accuracy required during the criterion trials of each habit was such that the ratios of the difference in percentage of trials in which the animal chose the token-baited container and the percentage expected by chance response (no differential reinforcement), divided by the standard error[10] of such a difference, were 4.3, 3.0, and 2.1 for Moos, Frank, and Bula respectively. At the poorest there could be but some 4 chances in 100 that the criterion fulfilling score was no better than chance.

The complete data on these habits are summarized in table 4. It is significant that both Frank and Moos each learned half or more of the ten habits in the minimum number of trials allowed by the criterion (40 trials), showing that the criterion was really too low for the total trials in each case to be representative of the animal's proficiency. Besides the mean trials to criterion,[11] another index of improvement from the first few reversals to the later ones is provided by the tabulated means for habits 1–5 and 5–10. We find that both animals showed definite improvement. The progressive improvement under the condition of token-incentives is evident when the first half and last half of individual habits are analyzed for relative accuracy of choice. The critical ratio[12] of the difference from first half to last half of total trials to criterion, for all habits, is 5.7 for Frank and 9.4 for Moos. Each habit but the first really began with the handicap of a definitely established position preference for the opposite container! The time records show a marked initial reduction in mean time per trial after the first one or two habits, with a fairly even rate for succeeding habits. The means for habits 1–5 as compared to those for habits 6–10 show a decrease in time per trial. Since the increase in rate of response occurred chiefly in the earlier half of the training, it might be suggested that this change may have been chiefly due to early general motor improvement and

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[10] All "standard errors" here and to be cited below were obtained by the formula: $\sigma_{\text{diff.}} = \sqrt{pq/n}$ from Yule (1936, p. 257).

[11] "Trials to criterion" here and later always means trials to, and including the criterion trials.

[12] Critical ratio = difference/$\sigma_{\text{diff.}}$

TABLE 4

Two-position learning with food-tokens

Trials to criterion, per cent correct first choices for first half and last half of each habit, and mean time per trial.

HABITS*	FRANK				MOOS			
	Trials to criterion	Per cent correct		Mean time per trial†	Trials to criterion	Per cent correct		Mean time per trial†
		1st half	2d half			1st half	2d half	
1	80	73	85	45	60	67	90	‡
2	60	60	93	12	80	53	98	39
3	40	85	100	12	60	80	97	14
4	40	85	95	13	40	90	100	16
5	40	85	100	14	40	80	100	13
6	40	95	95	18	60	83	97	13
7	40	95	100	15	60	83	97	13
8	40	90	95	24	40	95	100	14
9	40	95	100	15	40	85	95	14
10	40	85	90	29	40	95	100	18
(Means)								
1–5	52	75.4	93.1	22.7	56	70.7	96.4	22.6
6–10	40	92.0	96.0	20.0	48	87.5	97.5	13.8
1–10	46	82.6	94.3	21.5	52	78.5	96.9	17.4

HABITS*	BULA		
	Trials to criterion	Per cent correct	
		1st half	2d half
1	60	60	83
2	60	27	70

* The first position learned was right for Moos and Bula, left for Frank; succeeding habits were the position opposite to that of the preceding habit.

† To nearest second.

‡ Records incomplete.

adaptation to competing stimuli rather than to any increase in the general motivational effectiveness of the food-token. No differential response toward the tokens at these two different times would indicate such a motivational increase specific to the tokens, nor was this increased rate due only to reduction in number of errors with their attendant second choices.

Bula's unusual behavior, discussed under problem A, interfered increasingly with the experimental routine. Upon occasions the experimenter met with refusals to return to cage B from cage A for the second session of learning trials in a period. She frequently handled the choice-compartments violently, often refused to remove tokens, replaced in either container one or more tokens previously secured, or even threw tokens violently from the cage.[13] At times she was reluctant to leave the living cage to go to the experiment. This was in extreme contrast to the eagerness with which Bimba, Frank, and Moos regularly responded to the same procedures. Bula was finally abandoned as a subject in this experiment, even though she was working well alone as a subject in at least one other concurrent investigation under another experimenter. By nature uneasy when alone, her timidity may have appeared in greater degree when receiving tokens rather than the customary food. The latter might ordinarily have some quieting effect by reason of the preferred activity of eating.

C. Five-position learning, food-tokens or food

In this problem it was sought not only to demonstrate the learning with food-token incentives of more complex position habits than those of problem B, but also to compare the relative efficacy of direct food reward with that of the food-token in the same problem (figure 3, upper right). The comparison of food versus food-token incentives was carried out with Frank and Moos; Bimba was used as a third subject with only food-token incentives, no food ever being received in the learning apparatus.

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[13] The following excerpt from the Laboratory notes suggests that this emotional behavior may also occur in the usual food-rewarded experimental situation:

"... the barking of dogs and meowing of cats on the floor below ... interfered with experimentation on Bula and Alpha ... their uneasiness was expressed by whimpering (sometimes screaming), by refusing to work, or pacing around the cage. On all such occasions their discontent was expressed most violently not after they had completed a response (perhaps a failure, without reward), but rather during my preparations for the next setting and at the moment of presentation. Occasionally, as if sensing my desire that they work, they would make a hasty pull (if it was correct Bula often did not take the lure [food], or threw it on the floor) and then, whether they made a mistake or not, intensify their protest."—Nissen (March 1934).

After their trials for preliminary adaptation, Frank and Moos were each given two different procedures for learning the habits (described under procedures, pp. 17–19, above). Moos first received the "learning to criterion" procedure and later the "no criterion" or equal periods procedure. Frank was given these same types of training but in the reverse order. Bimba was given only the equal periods technique, and with no trials for preliminary adaptation.

Learning to criterion. Curves for the learning to a criterion by Frank and Moos are presented in figure 6. Each habit, whether with direct food or with food-token incentives, was learned to a criterion of first choice of the baited container (80 per cent during the 20 criterion trials), which would exceed chance accuracy[14] by a difference whose ratio to the standard error of the difference was approximately 4.7. Table 5 shows these scores for each habit and for each animal.

It is apparent that Moos learned three of the five habits with token-incentives in the minimum number of trials to criterion. This is especially significant because during the complete learning trials of each of these three habits, which involved abandoning the previously established preference for a different position, only tokens and no food were received. The tokens were, therefore, not only the sole indicators of the correct response but must have possessed sufficient incentive value to bring about the full establishment of each of those habits *before any food was received.* The nature of this incentive value will be discussed in a later part of this report.

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[14] Assuming that chance responses would give a score of only 20% for any single position. This, of course, is true only if the criterion is applied to *all* the positions, for they are by no means of equal difficulty for the animal.

From table 5 it may also be seen that there is general improvement in accuracy for both animals from the first half to last half of each habit with either type of incentive. It is quite evident for this problem, as for the simpler two-position problem, that the chimpanzee will readily learn such habits with food-tokens as differential reinforcing agents, even when a definite preference for another position has been previously established.

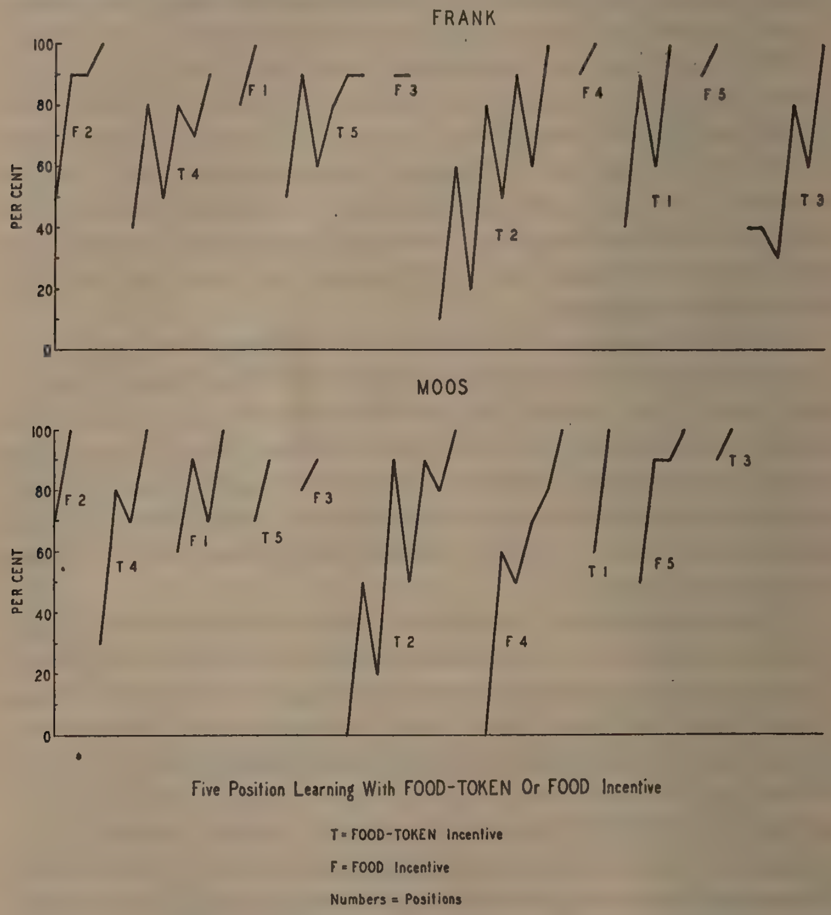

FIG. 6. CURVES FOR FIVE-POSITION LEARNING TO CRITERION

Each point represents per cent choice of baited container in ten trials

Differences in the ease with which particular positions are dis-

TABLE 5

Five-position learning to criterion with food or food-tokens

Showing serial order of habits, trials to criterion for each type of incentive, per cent correct first choices for first and second half each habit, and mean time per trial in seconds.

HABIT			FRANK				MOOS			
Box position	Serial order of habits	Incentive	Trials to criterion	Per cent correct		Mean time per trial	Trials to criterion	Per cent correct		Mean time per trial
				1st half	2nd half			1st half	2nd half	
1	3	Food	20	80	100	16	40	75	85	15
	8	Token	40	65	80	20	20	60	100	72
2	1	Food	40	70	95	16	20	70	100	18
	6	Token	100	44	80	19	80	40	80	35
3	5	Food	20	90	90	15	20	80	90	11
	10	Token	60	37	80	22	20	90	100	17
4	7	Food	20	90	100	15	60	37	83	12
	2	Token	60	57	80	23	40	55	85	24
5	9	Food	20	90	100	13	40	70	95	10
	4	Token	60	67	87	31	20	70	90	35
All	All habits	Food	Mn = 24	81.7	96.7	15.0	Mn = 36	61.1	88.9	12.8
		Token	Mn = 64	51.9	81.3	22.6	Mn = 36	54.4	86.7	34.7

HABITS	INCENTIVE	FRANK			MOOS		
		Total trials	Per cent correct, all trials	Mean time per trial	Total trials	Per cent correct, all trials	Mean time per trial
1–5	Food	80	86.3	15.5	80	82.5	14.6
	Token	120	72.5	26.7	60	73.3	27.3
6–10	Food	40	95.0	14.0	100	69.0	11.1
	Token	200	63.0	20.1	120	69.2	38.3
1–10	Food	120	89.2	15.0	180	75.0	12.8
	Token	320	66.6	22.6	180	70.5	34.7

criminated and learned may have favored certain habits, so we can best be assured of positive learning only if all five positions are learned under each condition. Position 2 (numbering from the animal's right to left), for example, seemed especially difficult for both animals when learned with food-tokens; but it may have been because the preceding habit in each case was probably the easiest of all and learned with the preferred type of incentive. The irregular order in which the habits were presented made possible both a forward and reverse order of the types of incentives with each pair of habits appearing together in the series—except for the particular pair just mentioned. This could not be avoided within the limits of the number of habits learned. However, if similarities in relative difficulty of the positions for both animals are sought in the individual curves, it may be seen that such agreement is infrequent. It appears from the grouped data of table 5 that, despite any such differences of score expected by the positional nature of the containers or sequence of habits, the food-rewarded habits show a marked superiority over the token-rewarded habits for one animal, Frank. Little or no such superiority is demonstrated for the other animal, Moos. This is true both in number of trials required to reach the criterion and in the per cent correct first choices of the rewarded container, taking the habits either singly or together.

It is possible that, because Frank came to this learning to criterion after considerable training in the same apparatus with the equal periods technique while Moos did not have that previous experience, Frank may have developed a more pronounced preference for direct food rewards. The score differences between food and token, when compared for the first five and last five habits, indicate the possibility of such an increase in difference for Frank as training progressed; but it is apparent that there was actually a decrease in difference for Moos. The critical ratios of difference to standard error of the difference between food and token accuracy for all trials reflect these individual characteristics of the two animals.[15]

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[15] Critical ratio of this difference: Frank = 5.8; Moos = 0.96.

When the time records are examined, we find a somewhat different case: both animals show a definite difference in rate of response in each type of incentive situation, and Moos showed even greater difference than Frank. General observations of the animals corroborated this quantitative difference in rate of response. The token-incentive periods were characterized by a great deal of extra play activity between trials, prolonged hesitation before coming to the grille to respond, frequent distraction, and hooting at outside sounds which seemed to be ignored during the food-rewarded periods. The upper half of table 8 summarizes the times taken by each animal for the completion of experimental periods under each condition of incentive. The critical ratio of the difference in duration of food and token periods indicates that the difference for both animals could hardly be a chance difference.[16]

A not infrequent bit of behavior for both animals during the early trials of each token-incentive period was to go beyond the point of correctly choosing the baited container and to open each of the other compartments as if in search of the direct food reward. In such cases, the token may or may not have been removed when the correct container was first opened. This behavior had not occurred during the two-position habits, where no food was ever received in the learning apparatus; and the practice dropped out during the subsequent training on other problems in which only food-tokens were the incentives. When direct food was the reward, the animals never chose additional compartments after correct response.

Equal periods technique. Under the condition of alternate equal periods of food- or token-incentive trials, a different habit each period, all three animals, Frank, Moos, and Bimba, gave clear evidence of learning with the food-token incentives. This part of the data is presented in tables 6 and 7. Each of the three animals showed a progressive increase in number of first choices of the token-baited container when proceeding from the first half to last half of each session for four out of five habits, as well as a reliable increase in the composite mean from first to second half of all habits with that type of incentive

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[16] Critical ratio of this difference: Frank = 5.2; Moos = 3.3.

TABLE 6

Five-position learning with food or food-tokens for equal periods

Serial order of positions and incentives, per cent correct first choices for equal periods under either type of incentive, and mean time (seconds) per trial.

BOX POSITION	SERIAL ORDER OF PERIODS	INCENTIVE*	FRANK		MOOS		BIMBA	
			Mean per cent correct†	Mean time per trial	Mean per cent correct†	Mean time per trial	Mean per cent correct†	Mean time per trial
1	8, 13	Food	73	16	95	13		
	3, 18	Token	40	23	80	41	54	51
2	1, 16	Food	35	17	83	13		
	6, 11	Token	43	19	78	67	16	55
3	5, 20	Food	68	17	88	15		
	10, 15	Token	43	22	88	15	56	43
4	12, 17	Food	93	16	90	11		
	2, 7	Token	45	18	58	40	63	50
5	4, 9	Food	83	16	83	12		
	14, 19	Token	68	23	73	12	60	46
Periods 1–10.....		Food	55.0	16.3	89.0	13.8		
		Token	38.0	19.7	72.0	44.9	21.5	63.7
Periods 11–20....		Food	85.0	16.7	86.0	11.8		
		Token	57.0	22.6	78.0	24.8	78.0	35.3
All, 1–20......		Food	70.0	16.5	87.5	12.8		
		Token	47.5	21.1	75.0	34.9	49.8	49.0

* For Bimba, tokens alone were used; serial order of positions remained same. For example, position 1 was baited with tokens during periods 3, 8, 13, and 18.

† For Frank and Moos, N = 40 trials in each case; for Bimba, N = 80 trials throughout.

(table 7).[17] The critical ratios of the difference between observed per cent correct first choices in both halves of all habits and the per cent which might be expected by chance alone[18] are 6.1, 13.2, and 9.3 for Frank, Moos, and Bimba respectively (table 6, token data only). This indicates that even when these positions are not learned to an established criterion, the general scores show, when all the positions are taken together, that the token incentives will elicit choice of the baited container with a frequency far exceeding a chance expectation.

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[17] Critical ratio of this latter difference: Frank = 5.6, Moos = 4.1, Bimba = 2.5.

TABLE 7

Five-position learning with food or food-tokens for equal periods

Per cent correct, first half and last half of each session*

BOX POSITION	INCENTIVE	FRANK		MOOS		BIMBA	
		1st half	2d half	1st half	2d half	1st half	2d hal
1	Food	45	100	90	100		
	Token	15	65	65	95	53	55
2	Food	20	50	65	100		
	Token	30	55	55	100	18	15
3	Food	50	85	80	95		
	Token	30	55	80	95	45	68
4	Food	85	100	80	100		
	Token	50	40	30	85	45	80
5	Food	65	100	65	100		
	Token	45	90	85	60	58	63
All positions....	Food	53.0	87.0	76.0	99.0		
	Token	34.0	61.0	63.0	87.0	43.5	56.0

* For Frank and Moos, each figure represents per cent correct in 20 trials, since halves of two 20-trial sessions are included for each condition. For Bimba, this number is doubled, since halves of four 20-trial sessions are included in each case.

In the comparison of food incentive with token incentive, the data of Frank and Moos are presented in tables 6, 7, 8, and graphically in figure 7. Unlike the results for the technique of learning to a criterion, differences with the equal periods technique are consistent for both animals in demonstrating the greater efficacy of the food incentive over the token incentive. This can be seen in the large differences in per cent correct for all habits with each type of reward (table 6),[19] in the relative increases from first half to last half of each experimental session (table 7),[20] and in the difference in duration of experimental periods (lower half of table 8).[21] General observations on the behavior of the subjects during sessions with each type of incentive were practically the same as during the procedure of learning to criterion. The "searching for food" after correct token choices, as well as long waits, distraction, and unusual play activity were common, and chiefly within the first few trials in any token-session. During this "no criterion" training, Moos broke one or two food-tokens with his teeth during each learning session, something he had not done during previous learning periods. He was not given food in exchange for fragments of

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[18] Assuming chance score to be 20%. (See above, p. 39, footnote 14.)

TABLE 8

Five-position learning with food or food-tokens

Mean duration in seconds of all experimental periods with each type of incentive, learning to criterion or for equal periods.

METHOD OF PRESENTATION	INCENTIVE	FRANK			MOOS			BIMBA		
		Mn	σ	N*	Mn	σ	N	Mn	σ	N
To criterion	Food	300	21	6	257	59	9			
	Token	452	112	16	693	394	9			
Equal periods	Food	330	26	10	256	49	10			
	Token	422	73	10	697	545	10	980	500	20

* Number of periods; for all three subjects, there were 20 trials in each period, either method of presentation.

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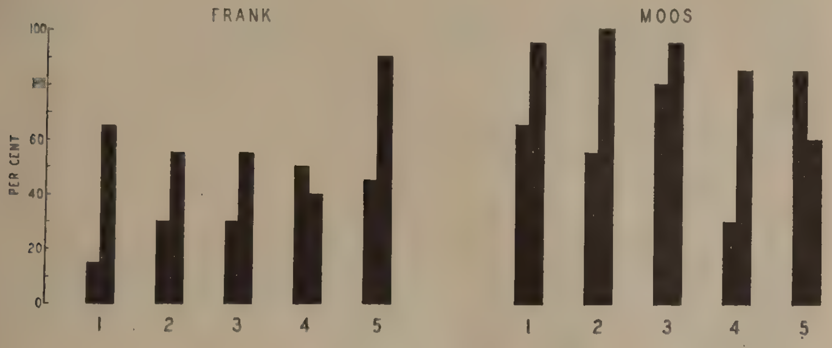
[19] Critical ratio of this difference: Frank = 4.7; Moos = 3.3 (N = 200 in each case).

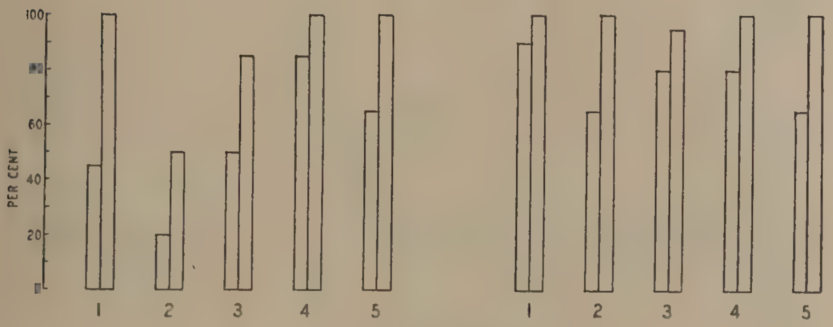
[20] Critical ratio of this difference, with *food reward*: Frank = 5.6, Moos = 5.2. Compare with the smaller critical ratios with *token-incentives*: Frank = 4.0, Moos = 4.1 (N = 100 in each case).

[21] Critical ratio of this difference: Frank = 3.8, Moos = 2.6.

tokens, and during subsequent training he desisted from the practice.



Learning With FOOD-TOKEN Incentive



Learning With FOOD Incentive

FIG. 7. COLUMN CHART OF FIVE-POSITION LEARNING WITH FOOD-TOKENS OR FOOD FOR EQUAL PERIODS

Left-hand column in each pair represents per cent choice of that baited container in the first halves of two 20-trial sessions; right-hand column, per cent in last halves of two 20-trial sessions.

D. *Visual size discrimination, food-tokens only*

The purpose of this problem was to test the food-token as a reinforcing agent for the learning of a fairly difficult visual discrimination problem. The stimuli selected were relatively unobstrusive outlines of circles which differed in circumference

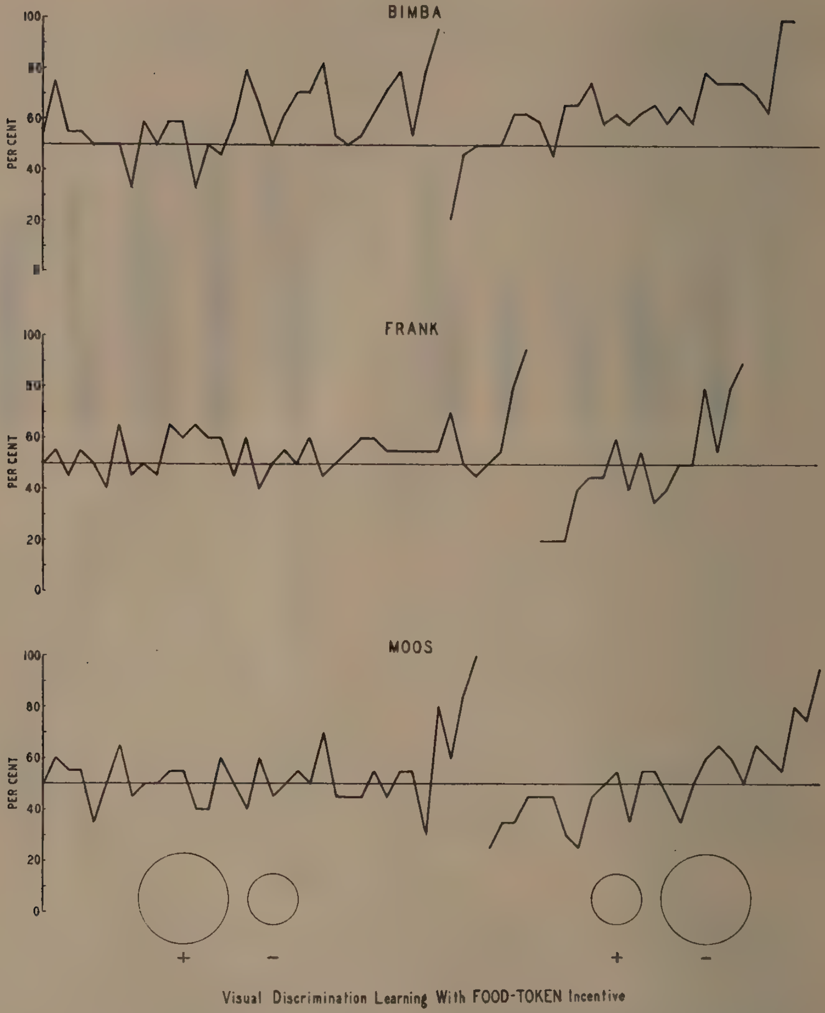


Fig. 8. Learning Curves for Visual Size-Discrimination

Each point represents per cent choice of token-baited stimulus in one experimental period: 24 trials (Bimba), or 20 trials (Frank and Moos).

(figure 3, lower left). This left size-cues as practically the sole basis of differentiation.[22]

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[22] Various experiments have indicated that such a limitation of differential cues, and the use of unfilled rather than solid figures, should make this discrimination a difficult one for the chimpanzee.

The graphs in figure 8 present the complete learning curves for the three subjects. All learned this problem. The incentives were food-tokens throughout. The data are summarized in table 9, in which is included not only the total number of trials and per cent correct first choices of the token-baited stimulus for each half of the habits, but also the time records.

The criterion of mastery which each of these animals attained in the last 40 trials of each habit (85 per cent first choices of the token-baited stimulus) represents a difference from a 50 per cent chance expectancy with a critical ratio, to the standard error of the difference, of approximately 3.6. This in itself is indubitable evidence of learning of the habits. The first half trials of the first habit did not differ significantly from chance for any of the animals, so it cannot be inferred that the first discrimination habit was favored by any preference for the positive stimulus which the animal might have brought to the situation.

TABLE 9

Visual size-discrimination learning with food-tokens

Showing for the first and last half of each habit the number of trials, per cent correct first choices of the baited stimulus, and mean time per trial (seconds).

HABIT	FRANK			MOOS			BIMBA		
	Number of trials	Per cent correct	Mean time per trial	Number of trials	Per cent correct	Mean time per trial	Number of trials	Per cent correct	Mean time per trial*
Large vs. small circle:									
First half trials	390	53.1	39.2	350	50.6	60.9	324	56.5	23.8
Second half trials	390	57.7	29.5	350	57.1	30.5	324	67.0	22.9
Total trials to criterion	780	55.4	34.3	700	53.9	45.7	648	61.7	23.4
Small vs. large circle:									
First half trials	170	37.1	26.2	270	40.4	37.8	336	55.4	18.1
Second half trials	170	60.0	22.1	270	60.7	96.2	336	72.0	17.9
Total trials to criterion	340	48.5	24.1	540	50.6	67.0	672	63.7	18.0

* *Net time* without the experimentally added inter-trial time, or delay before second choice in error, which were included in the later portion of Bimba's training.

Marked improvement from first-half to second-half trials was shown, however, by all subjects.[23] By the time the criterion of mastery was met by the three animals, each had chosen the token-baited stimulus considerably more often during the entire training, as well as during the last half trials, than would be expected by chance factors alone.[24]

The second habit was learned by each of the three animals in approximately an equal or smaller number of trials than the first habit, and this in spite of the fact that the second habit began with the handicap of an established preference for the opposite stimulus. From accuracy scores near or below 50 per cent for all the first half trials of this second habit, the animals showed significant increases in first choices of the positive stimulus with the completion of the second half trials until the criterion was met.[25] Except in the case of Bimba, the total choices for the entire second habit were in each case not significantly above 50 per cent, a difference from the learning of the first habit which is attributable to the depressed level of accuracy immediately following reversal. Considering this latter fact and that of the above-mentioned general improvement, there can not be inferred any decrease in effectiveness of the food-token as incentive to learning. Furthermore, the differences between per cent choices of the token-baited stimulus during the second half trials and a chance level of accuracy were significant.[26] These facts, together with the meeting of the same high criterion of mastery as was required for the first habit, make it clear that general improvement and final mastery of a relatively difficult discrimination will occur when food-tokens are the differentiating rewards.

The time records show a general increase in rate of response from first half to last half of each habit and from first habit to second. One notable exception is to be seen in the second half trials of the second habit for Moos. Here the mean time per trial was excessively high and can be ascribed to his general behavior at this time. During the last fifteen experimental periods, most of which were at a near 50 per cent level of response, this animal began to display a definite reluctance to respond for long intervals of time during the first five or six trials of nearly every period. During long waits of from a few minutes to the greater part of an hour, Moos would retreat to the rear of the restraining cage and either burst into boisterous play activity or lapse into complete quietude. The former was characterized by violent bumping of the cage, biting the tokens he had collected, vocalizing, or rolling about on the floor; the latter progressed from a nearly continuous, nervous scratching of the self while seated in a corner, to yawning, dropping the head in a posture of dozing, and staring fixedly toward the floor, motionless the while. During the animal's continued refusal to respond, the drop-door of the learning apparatus was lowered and lifted only at two- or three-minute intervals; when he finally returned to work, Moos generally completed the period of trials with dispatch. A milder display of this behavior had occurred during the early sessions of the first visual discrimination habit. Frank often delayed between trials, but the delays were short and well distributed over the whole extent of training.

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[23] Critical ratio of increase: Frank = 2.2, Moos = 1.9, Bimba = 2.8.

[24] Critical ratio of this difference above 50%, for the last half trials: Frank = 2.2, Moos = 1.9, and Bimba = 4.5.

[25] Critical ratio for this increase: Frank = 4.3, Moos = 4.8, and Bimba = 4.5.

[26] Critical ratio for this difference above 50%: Frank = 1.9, Moos = 2.5, Bimba = 6.0.

It would be difficult to explain the incidence of this peculiar behavior, especially to determine whether it be due to the difficulty of the learning problem, regardless of the condition of token-reward, or to fluctuations in the general motivational value of the food-tokens. The latter might be disturbed by such a factor as contemporaneous experimentation in which direct food, or more preferred food, was being employed. A complex of factors, including the animal's individual general variability, may have been involved. Moos' attempts to withdraw from the learning problem, whether by activity or inactivity, seemed to indicate that the problem was quite difficult—at least for the particular amount of reward being used. The tokens, whenever discrimination choices were made, were not always removed from the apparatus by the animal. This was also true of both Frank and Bimba, who, unlike Moos, had not preceded this problem with one involving direct food reward; but for all three animals this behavior almost invariably occurred only during continued failure to solve the problem. This further suggests difficulty of the problem or impatience to quit the situation, for it is a known characteristic of the chimpanzee, even when hungry and in a direct food rewarded problem, to refuse to accept the food upon successes if failures are too frequent or the problem insolvable. Very infrequently each of the animals, during this particular discrimination problem, would choose the correct stimulus, and, instead of removing the token, would replace another previously received token there in the compartment with the first! This behavior never persisted, and hence could not be looked upon as definite anticipatory "exchanging" of the token, but rather as an exaggeration of the refusal of reward.

E. Training of preferential differentiation of food-tokens and non-food-tokens

The purpose of this part of the investigation was to introduce a token, which differed in color from the food-token previously used by the animal, and which would not yield food when deposited in the food vender; and to train the animal, if necessary, to differentiate preferentially between these two types of token in a variety of situations. This new kind of token was named the "non-food-token" to distinguish it from the "food-token," which was always exchangeable for food.

Frank and Moos were the subjects throughout this and all the later problems which employed the non-food-token. A summary of their training in the differentiation of food-tokens and non-food-tokens is presented in table 10. The types of presentation have been described in the section on procedure (see above p. 22). Since previous training with food-tokens had reached the point of grouped exchange, the first presentation adopted was simply a repetition of this working for the food-tokens singly until a group had been collected. This was a check on the grouping limits of the animal. It can be seen that the perform-

TABLE 10

Training differentiation of food-token and non-food-token

Summary for each animal of number of tokens presented, taken, and exchanged under each type of presentation. FT = food-token; NFT = non-food-token.

TYPE OF PRESENTATION*	EXPERIMENTAL PERIODS	NUMBER OF TOKENS PRESENTED		NUMBER OF NFT		RATIO: NUMBER NFT EXCHANGED TO NFT PRESENTED
		FT	NFT	Taken	Exchanged	
	Frank					
A. Pull-in, free choice						
Singles, groups of 30	1–9	120	0			
immed. exch.		24	32	32	32	1.00
groups (5)		45	45	45	45	1.00
Pairs, immed.		38	38	38	38	1.00
Singles, immed.	10–18	35	29	29	23	0.79
groups (2–10)		73	38	28	20	0.53
Pairs, immed.		44	44	16	3	0.07
groups (5–10)		68	68	17	3	0.04
B. Pull-in, partition choice	19–24					
Singles, immed.		5	0			
Pairs, immed.		26	26	2	2	0.08
groups (10–18)		151	151	1	0	0.00
Totals......................	1–24	629	471	208	166	0.35
	Moos					
A. Weight-lift and pull-in	1–4					
Singles, groups of 30		120	0			
B. Pull-in, free choice	5–12					
Singles, immed.		40	36	33	17	0.47
groups (3–10)		98	100	99	79	0.79
Pairs, immed.		39	39	37	7	0.18
groups (5–7)		15	15	11	3	0.20
C. Pull-in, restricted choice	13–20					
Singles, immed.		15	14	5	2	0.14
groups (5–9)		19	17	7	0	0.00
Pairs, immed.		131	131	23	14	0.11
groups (2–13)		70	70	22	18	0.26

TABLE 10—*Concluded*

TYPE OF PRESENTATION*	EXPERIMENTAL PERIODS	NUMBER OF TOKENS PRESENTED		NUMBER OF NFT		RATIO: NUMBER NFT EXCHANGED TO NFT PRESENTED
		FT	NFT	Taken	Exchanged	
		Moos—*Concluded*				
D. Pull-in, partition choice	21–27					
Singles, immed.		4	0			
Pairs, immed.		60	60	1	1	0.02
groups (4–18)		160	160	18	18	0.11
Totals....................		771	642	256	159	0.25

* For explanation of these types of presentation, see text, p. 54ff.

ance in each case was equal to or better than the best performance at the end of the previous work-training (cf. table 3, p. 30). The intervening practice with the learning problems, where the tokens were never collected in groups exceeding 20, but were collected over a longer period of time, seemed to have raised the general limits of grouped work for tokens.

The non-food-tokens were first presented singly, in mixed order with food-tokens, for immediate exchange. The rate was regulated so that there would be no confusion through depositing non-food-tokens at nearly the same time as receiving food for food-tokens. Series of non-food-tokens alone were also given, first for immediate exchange and later intermixed with series of food-tokens, both types in small groups for delayed exchange.[27]

The next step was to pair one of each type of token, allowing immediate exchange of the chosen tokens, but later requiring a group of choices before exchange. "Free choice" refers to the placing of both types of token in the pull-in tray, but without the swinging partition which was later used in "partition choice."

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[27] A marked difference in rate at which the animals would work for either food-tokens or non-food-tokens appeared early in training. Typical times for pulling in 10 of each kind of token are (Moos): FT = 1 min., NFT = 6 min.; FT = 1 min., NFT = 2½ min.; FT = 1 min., NFT = 4 min.

In this "free choice" the animal could remove either or both tokens; in the "restricted choice" the apparatus was the same, but when the animal had made a choice, a second choice was prevented by removing the other token from reach. The device of a swinging partition in the choice tray accomplished this last purpose without the necessity of the experimenter's intervention.

Both animals at first exchanged either type of token with almost equal readiness. Moos, however, very soon began to refuse the non-food-token when presented alone, or to choose the food-token in the paired situations. Frank did not show this differentiation until after much more training and unrewarded "exchanges" of the non-food-token. This is indicated by the "ratio of the number of non-food-tokens exchanged to the number of non-food-tokens presented," which is tabulated in the last column of table 10. It will further be seen that even when accepted, the non-food-tokens were not always exchanged. Moos would drop them to the floor; Frank would continue to hold uncashed non-food-tokens, and very readily gave them to the experimenter at the end of the period. The continual decrease in this ratio for Frank is in striking contrast to the variability for Moos. The latter showed differentiation earlier in the training, but it was less stable than Frank's differentiation, when that was finally established.

The criterion of differentiation was not fixed, but both animals in approximately an equal number of periods attained a proficiency marked by no more than one choice of a non-food-token in 36 paired presentations with the food-token. Exchange was allowed only after 18 choices in each group. In general, it was far more difficult to establish a differentiation which persisted during the *grouped* collecting and exchange procedure than when immediate exchange was allowed after each choice. For this reason, and in view of the necessity of employing a grouped procedure during learning trials on problems to follow, the training was prolonged until the animals reached such proficiency under those conditions.

The extent to which this trained preference for the food-token was retained during the later work was tested by the "retest periods" (see procedure, p. 23), which were given only when there could be any doubt that the animal might begin a new habit with the necessary differentiation between food-token and non-food-token. As long as both types of tokens were being received and the resultant behavior of the animal was such as to demonstrate a clear preference, the learning and exchange periods themselves served as sufficient check. However, after periods in which the food-token was the only type of incentive, separate retest periods were conducted before proceeding to a problem involving both types of tokens. That this was not necessary as reinforcement of the differentiation between the two types of token was shown by the continued 100 per cent choice of food-token by both animals, even though the grouped procedure (two sessions of 18 trials) was employed during retests.

F. Visual color-pattern discrimination, food-tokens only, or food-tokens and non-food-tokens

The immediate purpose of this part of the investigation was to test the strength of the non-food-token as a reinforcing agent in learning. Over a long period of training this type of token had never yielded food to the animal upon exchange; furthermore, the food-token was definitely preferred to the non-food-token in a variety of choice-situations. Any comparison of the relative reinforcing strengths of these two types of token should indirectly give evidence of the rôle of the food-token's association with food in determining that strength. Likewise it should be possible to draw some conclusions from such a comparison as to the existence of any "intrinsic" reinforcing strength of the non-food-token, any goal- or reward-value of a token which is neither directly associated with food, nor a means to food.

By using the learning of many successive reversals of a simple discrimination habit it was hoped to obtain a series of habits of nearly equal difficulty. Certain of these habits were to be learned under one condition of incentives: a food-token as sole incentive for response to one particular stimulus; others were to be learned, if such learning occurred, under another condition of incentives: a food-token for responding to one stimulus, a non-food-token for responding to the other. These two methods of baiting the stimuli were mixed in a random but balanced order throughout the series of habit reversals, so that neither condition would be unduly affected by any large progressive error, such as the possible continuous decrease in difficulty with repeated reversals. It was expected to compare the relative efficacy of either technique of baiting by quantitative differences in the learning scores under each condition. Qualitative observations of differential general behavior on the part of the animals would serve as supplementary data. This rather indirect method of obtaining data on the efficacy of the non-food-token was dictated primarily by the obvious difficulty, and perhaps impossibility, in eliciting responses day after day with non-food-tokens alone.

Frank and Moos were the subjects for this problem. The sequence of habits each learned, together with summary data on their learning scores for each habit, are presented in table 11. The lettered classification of each habit after the first, according to "type of shift," was for purposes of later analysis. This refers not only to the type of incentives used for the given habit, but also indicates the type of incentives used for the immediately preceding habit.[28]

From the successful results of problem D (above) it was assumed that the animals would surely learn this new discrimination with food-tokens alone. The first habit was therefore presented with the two-token technique (food-token and non-food-token), in order to see at once whether any learning would occur when the stimuli were both baited. It could *not* be assumed, merely because the food-token was always preferred in choices with the non-food-token, that such preference was sufficient to establish consistent selection of the stimulus baited with food-tokens. Conclusive evidence that such learning of the food-

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[28] Such a treatment is necessary, since the score on any habit in such a series most probably depends not only upon the conditions prevailing at that time, but also, with diminishing weight, upon the degree of learning of each preceding habit. If the essential differences between those preceding habits are the very conditions whose effects we are studying, we must therefore take into account those conditions when appraising the learning score of the particular given habit.

TABLE 11

Sequence of color-pattern discrimination habits

Presenting for each habit the type of shift in incentives, the position of incentives, the number of trials the animal required to meet (and including) the criterion, and the number of correct responses. (+) = food-token; (−) = non-food-token; (0) = *no* token at all. "Green" and "red" are the stimulus plates baited as indicated.

HABIT NUMBER	TYPE OF SHIFT*	FRANK					MOOS				
		Position of incentives		Trials to criterion	Number of correct responses		Position of incentives		Trials to criterion	Number of correct responses†	
		Green	Red		1st half	2nd half	Green	Red		1st half	2nd half
1		+	−	108	30	45	−	+	360	100	120
2	D	−	+	180	36	61	+	−	396	86	113
3	B	+	0	180	42	70	0	+	162	39	60
4	A	0	+	252	55	86	+	0	180	43	68
5	C	+	−	198	26	75	−	+	270	34	86
6	B	0	+	180	52	63	+	0	198	48	81
7	C	+	−	72	16	32	−	+	108	24	39
8	B	0	+	36	13	17	+	0	54	19	26
9	A	+	0	144	29	52	0	+	144	33	51
10	C	−	+	180	33	71	+	−	252	45	86
11	D	+	−	108	25	40	−	+	162	28	65
12	D	−	+	216	52	87	+	−	126	13	52
13	D	+	−	144	34	58	−	+	108	33	43
14	B	0	+	144	44	60	+	0	72	18	30
15	A	+	0	144	33	61	0	+	108	28	48
16	C	−	+	108	37	47	+	−	216	66	79
17	B	+	0	72	24	30	0	+	252	65	97
18	A	0	+	72	15	35					
19	A	+	0	54	19	25	Total trials		3,168		
20	A	0	+	90	22	41					
21	C	+	−	126	29	45					
22	D	−	+	72	17	34					
23	B	+	0	72	29	33					
24	C	−	+	54	14	27					
25	D	+	−	72	25	31					
26	D	−	+	54	17	26					
		Total trials		3,132							

* Type A represents food-token only on present habit, food-token only on preceding habit; type B, food-token only on present habit, food-token and non-food-token on preceding habit; type C, food-token and non-food-token on present habit, food-token only on preceding habit; type D, food-token and non-food-token on present habit, food-token and non-food-token on preceding habit.

† First half and second half of total trials of each habit.

token associated stimulus would occur is furnished by the results of the second, if not of the first, habit. (It might be argued that simple preference for the particular stimulus color or pattern established the first habit in each case.) It may be seen from this same table 11 that a large number of such habits was learned, as well as numerous habits where the food-token was the sole differentiating incentive. In every case, when the criterion was met, the stimulus plate baited with food-tokens had been selected during the last 36 trials with a frequency exceeding the 50 per cent, or chance, level by a thoroughly reliable difference.[29]

If the number of correct responses for first half and last half of the trials in the various habits are compared, it is seen that in every case there was progressive improvement within each habit, regardless of type of incentives and without considering what final criterion was met. As further evidence of gradual improvement, as against any sudden learning during the criterion trials, one may examine the increases in accuracy, for each method of baiting, from first half of the experimental session to the second, in table 12. This is true for either fraction of the total trials to criterion. Critical ratios of these differences are given, and indicate high statistical reliability when the scores are grouped for all trials under each incentive condition.

While the comparative increases in accuracy within the sessions for each type of incentive are inconsistent for each half of the total trials to criterion, there is definite evidence that the absolute per cents are higher for the habits with food-tokens alone as incentives than for those employing both food-tokens and non-food-tokens as incentives. These differences in absolute per cents, as well as amounts of increase, are both marked and consistent when all trials are massed for each type of baiting. The direction of these differences would suggest, if the incentives of the present habit alone are considered, that the technique of food-token alone led to higher accuracy and greater improve-

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[29] The criterion of 12 out of 18 choices, followed by 17 out of 18 choices, equals 80.5% for 36 trials; critical ratio (diff. from 50%) = 2.88. If the last 18 trials are taken alone, 17 out of 18 becomes 94.4% and the critical ratio of difference from 50% is 3.41.

TABLE 12

Color-pattern discrimination learning with food-tokens only, or with food-tokens and non-food-tokens

Showing the per cent first choices of the stimulus baited with food-tokens in the first half and second half of the experimental session.* The habits are grouped according to the type of incentives for the present habit only. All habits, including the original and first reversal, are included in this table. FT = food-token; NFT = non-food-token.

	INCENTIVES FOR PRESENT HABIT					
	FT only		FT + NFT		FT only	FT + NFT
	Fraction of all trials with such incentives					
	First half trials	Second half trials	First half trials	Second half trials	All trials	All trials
Frank						
Total trials (both half-sessions).	720	720	846	846	1,440	1,692
Per cent correct:						
First half-sessions	55.8	65.9	53.9	65.7	60.9	59.8
Second half-sessions	62.8	79.5	62.0	71.4	71.2	66.7
Mean increase per session	6.9	13.6	8.0	5.7	10.3	6.9
Difference/σ_{diff}	1.9	4.1	2.4	1.8	4.2	3.0
Moos						
Total trials (both half-sessions).	576	576	990	990	1,152	1,980
Per cent correct:						
First half-sessions	57.4	59.4	49.6	52.1	58.4	50.9
Second half-sessions	70.2	69.2	52.5	67.0	69.7	59.8
Mean increase per session	12.8	9.7	2.8	14.9	11.3	8.9
Difference/σ_{diff}	3.2	2.5	0.88	4.8	4.0	4.0

* For Frank, each session (trials before exchange of tokens) = 18 trials; for Moos, each session (trials before exchange of tokens) = 36 trials.

ment in response to the stimulus thus baited, than did the technique of food-token on one stimulus and non-food-token on the other.

The time records, or rate of response, for each method of reward are presented in table 13. These show no significant or consistent differences in favor of one type of baiting or the other, whether the grouping of results is on the basis of the incentive conditions for the present habit alone or is subdivided in terms of the incentive conditions for the preceding habit. Neither are the changes from the first half of all trials under a given condition to the second half of all such trials large nor consistent from one animal to the other.

TABLE 13

Color-pattern discrimination learning with food-tokens only, or with food-tokens and non-food-tokens

Time summary, showing mean time (seconds) per trial under each condition of incentives, grouped according to incentives for present habit and incentives for preceding habit. The original habit and first reversal are omitted. FT = food-token; NFT = non-food-token.

INCENTIVES FOR PRESENT HABIT	INCENTIVES FOR PRECEDING HABIT	FRANK		MOOS	
		Total number of trials	Mean time per trial	Total number of trials	Mean time per trial
FT only	FT only	756	15.6	432	18.2
	FT + NFT	684	16.9	738	18.6
FT + NFT	FT only	738	16.0	846	18.7
	FT + NFT	666	14.2	396	19.8
FT only	Either type	1,440	16.2	1,170	18.5
FT + NFT	Either type	1,404	15.1	1,242	19.1
Either type	1st half, all trials	1,422	17.1	1,206	18.4
Either type	2d half, all trials	1,422	14.3	1,206	19.3

In table 14 the per cent choices of the stimulus baited with food-tokens is indicated for each half of the trials to criterion for each habit, grouped according to the type of incentives employed for the given habit and for the preceding habit. It is quite evident that for each of these conditions there were large increases from first half to last half trials of each group of habits, corroborating for entire habits the improvement we have already observed from first half to last half of the experimental sessions. The first two habits are omitted in the case of each animal's data,

TABLE 14

Color-pattern discrimination learning with food-tokens only, or with food-tokens and non-food-tokens

Summary of trials to criterion, and per cent correct responses, for first half and second half of habits grouped according to each of the four shifts of incentives from preceding habit to present habit. The original habit and first reversal are omitted. FT = food-token; NFT = non-food-token.

	TYPE OF SHIFT OF INCENTIVES			
	A	B	C	D
	Incentives for present habit			
	FT only	FT only	FT + NFT	FT + NFT
	Incentives for preceding habit			
	FT only	FT + NFT	FT only	FT + NFT
Frank				
Total trials	756	684	738	666
Mean trials to criterion	126	114	123	111
Per cent correct:				
First half	45.8	59.6	42.0	51.0
Second half	79.3	79.8	80.5	83.0
All	62.5	69.7	61.3	67.0
Moos				
Total trials	432	738	846	396
Mean trials to criterion	144	148	212	132
Per cent correct:				
First half	48.1	51.2	40.0	37.4
Second half	77.4	79.7	68.6	80.8
All	62.7	65.5	54.3	59.1

primarily because the initial habit began at a near 50 per cent level, instead of having begun, as did succeeding reversals, at a level considerably below 50 per cent. Since the first habit was learned under the two-token technique, including its data would have spuriously raised the earlier accuracy, and correspondingly reduced the amount of subsequent improvement, for that condition of incentives. Furthermore, Moos required a disproportionate number of trials to meet the criterion for the first two habits; these scores would have overbalanced the mean of trials to criterion for all habits with that two-token type of baiting.[30]

In spite of this treatment, the tabulated means of trials to criterion show no consistent or large differences for any type of shift in incentives from preceding habit to present habit. However, we find for both animals a similar rank order of per cent correct first choices of the food-token associated stimulus for all trials under the different types of incentives. The highest scores were made in the case of shift type B (food-token only in the reversal of a habit learned with food-token and non-food-token); and the lowest scores were made in the case of shift type C (two-token reversal of a single-token preceding habit). The two other types of incentive shift are intermediate between these extremes, the precise relations being different for the two animals.

From these last facts we may conclude that the more efficacious incentive condition is that involving the single food-token, for a habit so learned is more difficult to reverse[31] under the two-token technique than the single-token technique (cf. shifts A and C); furthermore, a habit learned under the two-token technique is more easily reversed by the single-token technique than with the two-token technique (cf. shifts B and D). These differences are consistent when the scores of both animals are compared. Table 15 presents these differences with their statistical reliabilities, and indicates in more striking manner the significant superiority of the incentive shift type B over type C, for both animals. The other differences, in comparing animals, vary in reliability but are consistent in direction except for the difference between types A and D, which might be expected to be ambiguous no matter which type of incentive technique, single-token or two-token, be more efficacious.

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[30] Moos required for each of the first two habits over two times as many trials as the mean trials to criterion of all remaining habits.

[31] Interpreting as "more difficult to reverse" the lower per cent accuracy during reversal, and conversely as "more easily reversed" the higher accuracy during reversal.

In order to summarize these differences between habits learned or reversed with each incentive technique, table 16 has been prepared. The habits are here regrouped, either according to the incentives used in the present habit only or according to the incentives used in the preceding habit only. This reveals again the significant difference in accuracy of choice of the stimulus baited with food-token when it alone is the incentive, above that accuracy which obtains when a non-food-token is associated with the alternative stimulus.[32]

TABLE 15

Color-pattern discrimination learning with food-tokens only, or with food-tokens and non-food-tokens

Differences and critical ratios between per cent correct all trials for the different types of incentive shift.* Original habit and first reversal are omitted.

TYPE OF SHIFT		FRANK			MOOS		
		B	C	D	B	C	D
A	Difference	+7.2	−1.2	+4.5	+2.8	−8.4	−3.6
	$\text{Diff.}/\sigma_{\text{diff.}}$	2.9	0.48	1.8	0.96	2.5	1.2
B	Difference		−8.4	−2.7		−11.2	−6.4
	$\text{Diff.}/\sigma_{\text{diff.}}$		3.3	1.1		3.7	2.6
C	Difference			+5.7			+4.8
	$\text{Diff.}/\sigma_{\text{diff.}}$			2.2			1.6

* The difference, in each case, is the shift-type in the left-hand column subtracted from the shift-type indicated at the top. For an explanation of these four types of shift, see table 14.

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[32] Judging from the lower half of table 16, it would seem that the incentives of the *preceding* habit determined to a greater, or more regular, degree the accuracy of choices or trials to criterion in the present habit. No special importance can be attached to this fact, for such a difference must be due to variability in absolute scores of the habits included in the different groupings, each habit in the analysis having been treated not only as "present" but as "preceding" habit.

The significance of these differences in accuracy of choice is heightened by the consideration that these percentages have all been extracted from "work-limit" data rather than "time-limit" (or what we have previously employed as "equal periods") data. By doing so we have included in our averages those added trials, in the case of the less efficient learning, which had to be completed in order to meet the very same criterion of accuracy as the more efficient learning. This methodological procedure has therefore tended to lessen those very differences which we have discovered.[33]

TABLE 16

Color-pattern discrimination learning with food-tokens only, or with food-tokens and non-food-tokens

Mean trials to criterion and per cent correct responses for habits, grouped according to similar incentives for present habit, or similar incentives of preceding habit. Differences between these combinations are presented. Original habit and first reversal are omitted. FT = food-token; NFT = non-food-token.

COMBINATION OF SHIFTS	INCENTIVES FOR PRESENT HABIT	INCENTIVES FOR PRECEDING HABIT	FRANK			MOOS		
			Total trials	Mean trials to criterion	Per cent correct	Total trials	Mean trials to criterion	Per cent correct
A + B	FT only	Either type	1,440	120	66.0	1,170	146	64.4
C + D	FT + NFT	Either type	1,404	117	64.0	1,242	177	55.8
Difference					2.0			8.6
Diff./$\sigma_{\text{diff.}}$					1.1			4.3
A + C	Either type	FT only	1,494	125	61.9	1,278	183	57.1
B + D	Either type	FT + NFT	1,350	113	68.4	1,134	142	63.2
Difference					−6.5			−6.1
Diff./$\sigma_{\text{diff.}}$					3.6			3.1

From the foregoing data and analysis we may safely conclude that there is positive evidence that these discrimination habits were more efficiently learned when a food-token was the sole incentive for the positive response than when the food-token incentive was supplemented with a non-food-token incentive for the negative response. Furthermore, the token associated with no food had insufficient positive reinforcing strength to effect learning of a response, when the antagonistic response was rewarded with a token associated with food. Nevertheless, the token associated *with no food*, when presented in the same general situation with a token associated *with food*, affected the animal's responses to the non-food-token associated stimulus *as if* it had a small degree of *positive* reinforcing strength. Two possible explanations of this latter effect are: (1) The non-food-token acquired positive incentive value by "irradiation" of reinforcing strength from the food-token, to which the former is similar and with which it is quite closely associated in the general situation, or (2) the mere presence of the non-food-token, either in adding one more intrusive factor, or perhaps in stimulating general frustration, reduced the effectiveness of the food-token.

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[33] On the other hand, to make a new analysis on the basis of equal numbers of trials selected from each habit would invalidate our comparisons, for the very existence of a greater number of trials for the less efficient conditions would tend to depress the scores of reversals employing the more efficient conditions.

Aside from the learning scores, certain of the qualitative observations of behavior may help to decide between these possibilities. The animals regularly responded in quite a different manner toward each type of token, particularly at the time of exchange after the learning trials. The behavior adopted by the animals varied from time to time, but in general both Frank and Moos followed this pattern: during the earliest sessions the non-food-tokens were all removed from cage B at the time of exchange and carried, with the food-tokens, to the exchange room. There the animal spontaneously separated them, leaving the non-food-tokens on the floor or depositing a few of them *after* the food-tokens had all been exchanged. As the learning periods proceeded, fewer non-food-tokens were taken to the exchange room; in 80 per cent of all periods for Frank, and in over 70 per cent of all periods for Moos, not more than one or two of the non-food-tokens were ever taken from cage B to cage A at the end of any session. Both animals frequently would collect the food-tokens in their mouths during the learning trials, dropping the non-food-tokens on the floor as they were taken from the learning apparatus. During 18 sessions toward the latter part of training, Frank threw over 45 non-food-tokens out of the learning cage, as soon as they were received from the apparatus; corresponding behavior for Moos was to flip non-food-tokens violently from the apparatus. Neither animal behaved this way toward food-tokens. This negativeness usually occurred when many errors were being made at the beginning of a new habit. Both animals displayed a great deal of restlessness throughout the training; typical of this was the frequent biting and breaking of tokens, food-tokens or non-food-tokens, indiscriminately, depending upon what tokens happened to be in the animal's mouth.

The differentiation retest periods which were given to Moos and Frank after habits numbered 4, 6, 9, 15, and to Frank after additional habits 20 and 23 (see table 11, p. 58), were further evidence of continued preference for the food-token. Moos chose the food-token in preference to the non-food-token at every choice presented to him; Frank chose the food-token in all but three choices, which occurred during his first retest session.

These observations on the qualitative behavior of the animals toward the two types of token, as well as the results of the preference tests during the retest periods, all seem to confirm the possibility mentioned above, that the presence of the non-food-token reduced the efficacy of the food-token, rather than that the non-food-token had acquired any positive reinforcing strength of its own. The animal may have treated it as an unavoidable obstacle or added complication in the problem for which the food-token remained the essential positive incentive.

Problem G, to follow, serves as further evaluation of the food-token as compared to the non-food-token, and will show that the explanation of apparent positive reinforcing strength of the non-food-token may not be as simple as the one suggested above.

G. Spatial delayed response, differential accuracy for food-tokens, non-food-tokens, or food; immediate or delayed exchange of tokens

The primary purpose of this problem was to test the efficacy of the food-token as an incentive for delayed responses. A further purpose was to test the relative efficacy of each of the three incentives: food-token, non-food-token, and food, in eliciting such behavior. And finally, it was planned to test the influence of time of exchange of tokens upon accuracy of delayed responses: to determine whether immediately exchangeable tokens are more efficacious incentives than those which can be exchanged only after a group has been collected.

Bimba, Frank, and Moos were the subjects for this problem. Bimba's data are not presented in detail, since she was used only in demonstrating the adequacy of food-tokens as incentives for delayed response. With Frank and Moos were carried out the extended comparisons of each type of incentive: food-tokens, non-food-tokens, and food.

Food-token only. Section 1 of table 17, as well as Bimba's results to be cited, may be presented as convincing evidence of the adequacy of food-tokens as incentives for delayed response. In each case the food-tokens were received in the delayed response apparatus, collected trial by trial, and exchanged as a group in the adjoining room after the entire session. There is no doubt that the food-token elicited delayed responses with general characteristics comparable to those typical of similar responses for direct food incentives. During 300 trials with food-token incentive and grouped exchange, Bimba averaged 91 per cent correct first choices of the rewarded container, the imposed delays ranging from 1 to 120 seconds, mode at 20 seconds, and averaging 25 seconds each; 10 out of 12 choices were correct with delays of 1 minute, 3 out of 4 were correct with delays of 2 minutes. While the food-token trials for Frank and Moos (table 17, section 1) were interspersed with non-food-token rewarded trials, we may assume that their performance for food-tokens alone would be equal if not better than that recorded under the conditions of such combination. Yet in this case we find that Moos and Frank chose the food-token rewarded container 78 and 82 per cent of 320 trials each, with average delays of 6 and 10 seconds respectively.

Food-token versus *non-food-token; delayed exchange.* The results of the comparison, with Frank and Moos, of the food-token and non-food-token as incentives for delayed responses are pre-

TABLE 17

Differential delayed response for food-tokens, non-food-tokens, or food

Showing for each comparison of incentives, and for each animal, the following items regarding successive groups of 100 trials: mean imposed delay for all trials, mean per cent correct for both types of incentive, per cent correct for each type of incentive, difference in per cent correct (subtracting score of second named incentive from that of first), and critical ratio of that difference.* Averages of all trials are also given. FT = food-token; NFT = non-food-token; F = food; d = delayed exchange; i = immediate exchange.

1. Food-token *versus* non-food-token; delayed exchange

TRIALS	FRANK						MOOS					
	Mean delay	Per cent correct			Difference	C.R.	Mean delay	Per cent correct			Difference	C.R.
		Both	FTd	NFTd				Both	FTd	NFTd		
	seconds				*per cent*		*seconds*				*per cent*	
1–100	6	87	86	88	−2	0.30	6	84	82	86	−4	0.54
101–200	14	76	80	72	8	0.94	7	69	80	58	22	2.4
201–300	10	81	96	66	30	4.1	7	81	84	78	6	0.77
301–400	10	86	96	76	20	3.0	5	79	72	86	−14	0.08
401–500	16	85	90	80	10	1.4	8	66	70	62	8	0.85
501–600	8	77	80	74	6	0.71	4	69	70	68	2	0.31
601–640	6	88	95	80	15		6	90	95	85	10	
1–640	10.3	82.3	88.4	76.1	12.3	4.1	6.3	75.6	77.5	73.7	3.8	1.1

2. Food-token *versus* non-food-token; immediate exchange

TRIALS	FRANK						MOOS					
	Mean delay	Per cent correct			Difference	C.R.	Mean delay	Per cent correct			Difference	C.R.
		Both	FTi	NFTi				Both	FTi	NFTi		
	seconds				*per cent*		*seconds*				*per cent*	
1–100	17	91	100	82	18	3.3	11	86	94	78	16	2.4
101–200	23	90	100	80	20	3.5	14	90	92	88	4	0.67
201–300	22	88	98	78	20	3.2	15	88	90	86	4	0.62
301–400	28	86	98	74	24	3.7	20	93	98	88	10	2.0
401–500	27	86	98	74	24	3.7	26	92	96	88	8	1.5
501–560	25	87	100	73	27							
1–500							17.4	89.8	94.0	85.6	8.4	3.1
1–560	23.8	88.0	98.9	77.1	21.8	8.4						

* Critical ratio (C. R.) = difference/$\sigma_{\text{diff.}}$

TABLE 17—*Concluded*

3. Food *versus* non-food-token; immediate deposit

TRIALS	FRANK						MOOS					
	Mean delay	Per cent correct			Difference	C.R.	Mean delay	Per cent correct			Difference	C.R.
		Both	F	NFTi				Both	F	NFTi		
	seconds				*per cent*		*seconds*				*per cent*	
1–100	26	84	100	68	32	4.9	26	83	94	72	22	3.1
101–200	18	80	96	64	32	4.4	22	88	98	78	20	3.2
201–300	20	80	96	64	32	4.4	26	85	94	76	18	2.6
301–400	20	86	98	74	24	3.7						
401–500	21	85	98	72	26	3.9						
1–300							24.5	85.3	95.3	75.3	20.0	5.1
1–500	21.1	83.1	97.6	68.4	29.2	9.4						

4. Food *versus* food-token; immediate exchange

TRIALS	FRANK						MOOS					
	Mean delay	Per cent correct			Difference	C.R.	Mean delay	Per cent correct			Difference	C.R.
		Both	F	FTi				Both	F	FTi		
	seconds				*per cent*		*seconds*				*per cent*	
1–100	38	93	94	92	2	0.40	26	91	90	92	−2	0.35
101–200	54	84	86	82	4	0.55	31	84	90	78	12	1.7
201–300	61	85	88	82	6	0.85	23	88	90	86	4	0.62
301–400	40	81	76	86	−10	1.3						
401–500	18	89	92	86	6	1.0						
1–300							26.4	87.7	90.0	85.5	4.7	1.2
1–500	42.0	86.4	87.2	85.6	1.6	0.52						

sented in table 17, section 1, and are summarized graphically in figure 9. While it is immediately apparent that both animals showed a fairly consistent difference in performance in favor of the food-token, it is striking that both did perform delayed responses for the non-food-token at a fairly high level of accuracy.[34]

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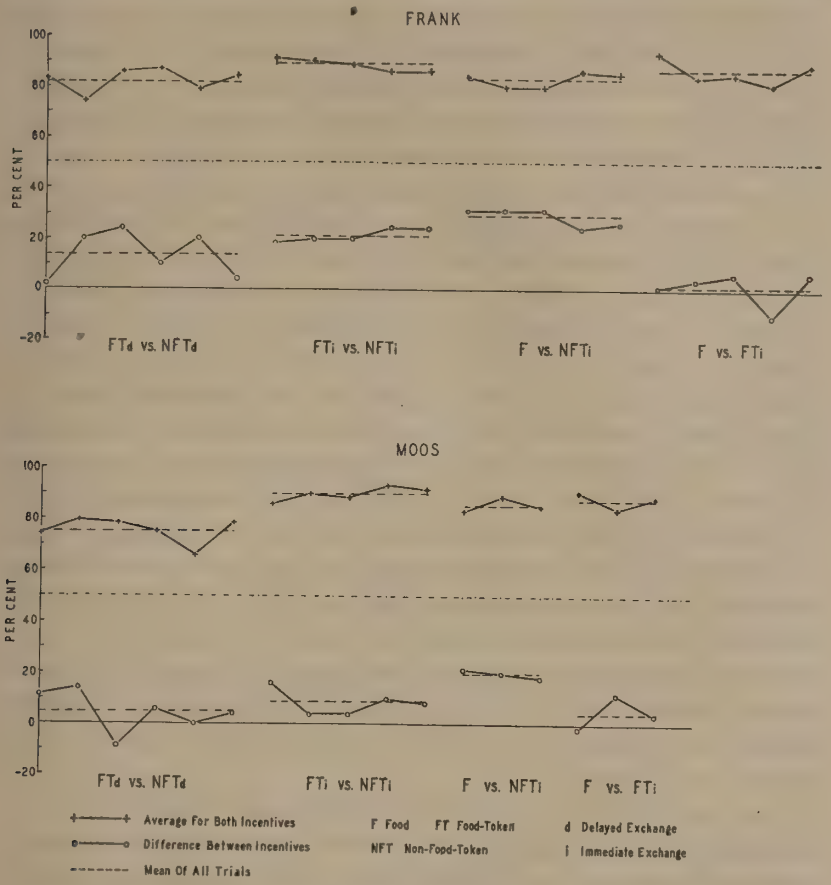
[34] Critical ratio of difference from 50% for the non-food-token: Frank = 7.1, Moos = 6.4.

We must grant that in this instance the non-food-token has some positive incentive value, for unlike the discrimination situation discussed above, to weaken the food-token's effect in the delayed response (as by generalized frustration) would not necessarily give to the non-food-token any apparent positive reinforcing



Differential Delayed Response

FIG. 9. COMPARATIVE CURVES FOR DIFFERENTIAL DELAYED RESPONSE

Each point on the upper (average) curves is the mean per cent correct in 100 trials both incentives; each point on the lower (difference) curves is the average of 50 trials with the first named incentive minus the average of 50 trials with the second named incentive.

strength. This may be explained in part on the basis of conclusions made in other delayed response experiments with quantitatively varying food incentives: the performance for the less preferred incentive is improved by presenting that incentive concurrently with a much more preferred incentive.[35] We may note that there is considerable variability in the relative accuracies with food-token and non-food-token; the difference varies from −14 per cent to +22 per cent (Moos) for different groups of 100 trials, and the reliabilities of this difference vary correspondingly. Nevertheless, the difference for all 640 trials is well in favor of the food-token for both Frank and Moos.

We may therefore conclude that accuracy of delayed response is an index of at least the relative strengths of these token incentives: accuracy of choice is higher for the preferred incentive (food-token), lower for the non-preferred incentive (non-food-token). Further conclusions as to what extent these accuracies may reflect the *amount* of that difference will be proposed below.

Food-token versus *non-food-token; immediate exchange.* The next series of trials for both animals was similar in procedure to the preceding, but one important new factor was included. This consisted of allowing immediate exchange or deposit of either type of token, as soon as it had been received from the delayed response apparatus. For this purpose the vender at the side of cage B was employed; it was opened for the animal after the completion of each trial.

Upon initiation of this new procedure, the general level of accuracy took a sudden and marked rise, especially for the food-token incentive.[36] The imposed delay was therefore increased, but the general level of response remained consistently higher than had obtained during the preceding series with delayed exchange of tokens. Furthermore, the difference in score between food-token and non-food-token became larger, and more consistently so, than in the preceding series. These effects are readily observed in figure 9. The reliability of this difference was higher, not only for all trials of the new series but also for the individual 100-trial groups (table 17, section 2). There were no negative differences for food-token scores minus non-food-token scores in any part of this series, such as had previously occurred.

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[35] Nissen and Elder (1935); Cowles and Nissen (1937), cf. especially their concept of "tolerance for errors" as a possible factor here.

Another explanation, which may possibly apply in this situation but would be difficult to demonstrate, is that an animal could respond differentially after the delay more readily to position of the incentive (left or right) than to specific non-spatial characteristics of the incentive (food-token *or* non-food-token). In other words, the animal might "remember" *where*, although he "forgot" *what* incentive.

[36] Compare the average accuracy for the last two groups of trials in table 17, section 1 (delayed exchange), with the corresponding scores for the first two groups in section 2 (immediate exchange).

Qualitative observations of the animals' behavior corroborated these facts. There occurred much less distraction, less play activity, and less hesitation between trials; and, in contrast, eager waiting at the response door and frequent pleasurable vocalizations indicated that the animals' general motivation had taken a new increase. Moreover, the animals' behavior toward the non-food-token at time of "exchange" altered remarkably after the first 100 or 200 trials: instead of depositing it immediately and skillfully, as they had done in the earlier series, both animals began to fumble with the token while placing it in the vender slot. Later this behavior became that of decided reluctance to deposit the non-food-token. Since the next trial was not begun until the token was disposed of, the animals did not adopt persistent refusals but only hesitated a great deal, occasionally throwing the non-food-token out of the cage rather than depositing it.

Here again we are confronted with the question: how can the non-food-token be as effective as it is, in eliciting accurate delayed responses, if such definitive antagonistic behavior was directed toward it? We find the food-token has increased greatly in efficacy, but the non-food-token also has increased some in efficacy, as compared with the previous series. This latter fact would further argue for the possibility of an irradiation effect from food-token to non-food-token. It appears that bringing the point of exchange spatially and temporally nearer to the animals' responses has not only emphasized the positive value of the food-token, but has also increased in lesser degree the apparent positive reinforcing effect of the non-food-token. It must be pointed out that the "forced deposit" of the non-food-token may have been an important difference between this series and the preceding series with delayed exchange, possibly resulting in a reduction of this increase for the non-food-token. Yet the marked behavioral negativeness to this procedure did not appear until the latter part of the series of trials with immediate exchange, and the large score differences not only began much earlier but also remained relatively unchanged during these behavioral changes.

This particular procedure was necessary in the immediate-exchange series, in order to equalize treatment of both types of token; for, to have allowed immediate exchange of the food-token and not of the non-food-token, would have left the latter in the possession of the animal during the other trials. In delayed exchange, both types of token were in the possession of the animal during the succession of trials.[37] These considerations, however, do not alter the principal fact of increased accuracy of response, especially for the food-token, when immediate rather than delayed exchange of tokens was allowed.

Food versus *non-food-token; immediate exchange.* The third series of delayed response trials was similar to the preceding series except for the substitution of direct food as the second incentive for comparison in place of the food-token. The non-food-token was still to be deposited after each trial in which it was received. The food incentives were raisins identical to those for which the food-tokens were previously exchangeable.

The purpose of this comparison between delayed response accuracy with direct food and the non-food-token as incentives was linked to the previous comparison and to a comparison yet to follow. By testing directly the relative efficacy of the non-food-token and *food* we should, by comparison with the foregoing test of relative efficacy of non-food-token and *food-token*, obtain indirect but important data contributing to a primary comparison of food-token and food. By this general procedure, it should be possible not only to place each of these incentives in a hierarchy of effectiveness as incentives in delayed response but also to arrange them in a rough quantitative scale of difference. As a complete empirical check on these relationships, a direct comparison of the food-token and food incentives was actually carried out in a final series of delayed response trials.

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[37] Since the animal practically never removed the non-food-tokens to cage A, no opportunity was given in the delayed exchange procedure for "forced deposit."

Table 17, section 3 presents the results of this series in which direct food and the non-food-token were compared. The accuracy scores show extremely large differences in the amount by which the food incentive excelled the non-food-token incentive. The food incentive scores were approximately equal to those of the food-token in the preceding series, but a marked fall in accuracy for the non-food-token trials occurred (10 per cent for both animals). The mean level of accuracy nevertheless did not drop significantly below that of the previous series. The food minus non-food-token differences in the present series have high critical ratios for every group of 100 trials, in contrast to the smaller and more variable ratios observed in the preceding series (especially for Moos). The mean delay was changed slightly from one series to the next, differently for each animal but affecting the mean accuracy of response in expected direction. Figure 9 summarizes graphically the changes in accuracy scores from the preceding series to this one.

The qualitative observations of the animals' general behavior in this food versus non-food-token situation indicated an extreme negativeness toward the non-food-token, marked especially by extremely slow and awkward depositing of it.

It would seem to follow from the data of this last series, when compared to the data of the preceding series, that if we are to explain the positive reinforcing effect of the non-food-token on the basis of irradiation from the preferred, food or food-token, incentive, we must account for the fact that there seems to be more irradiation of positive effect from food-token than from direct food itself! This suggests that similarity of incentives leads to more such irradiation than does the concurrent use of a much stronger incentive, and with this hypothesis we feel nearer the precise source of the non-food-token's positive strength.

Food versus *food-token; immediate exchange.* This final series of delayed response trials was carried out similarly to the last preceding series, except for the one important change of using the food-token incentive in place of the non-food-token incentive. This comparison, represented by the data in table 17, section 4, completed all possible paired comparisons of the three types of incentives with immediate exchange.

It may be seen that certain significant changes in the performance of the animals took place with the introduction of this new condition. The mean level of accuracy rose in spite of the experimenter's increasing the imposed delays, but the difference in accuracy between these incentives became statistically insignificant, and very small in comparison with the large differences which only recently had occurred between food and non-food-token incentives. In fact, a critical ratio of the difference in per cent accuracy for the two incentives, food and food-token, now indicated unreliability of the difference when averaged for the entire group of trials given each animal. Nevertheless, it must be noted that the difference was almost uniformly in favor of the food incentive. These last data are also summarized graphically in figure 9.

General behavior during this last series reflected for each subject the individual and combined effects of these two types of incentive. The food-token was regularly exchanged with promptness and skill; distractibility was at a minimum; impatience to respond for either incentive was the rule.

In order to arrive at more precise quantification of the relative effectiveness of these incentives in the delayed response, we have arranged equal numbers of trials of each of the respective series for each animal in table 18. If we now compare the differences obtaining in each case of immediate exchange,[38] we may arrange the three incentives on a linear scale for each animal, thus:

$$\text{Frank}\quad \frac{\text{NFT}\ldots\ldots 21\%\ldots\ldots\ldots\text{FT}.2\%.\text{F}}{\ldots\ldots\ldots\ldots\ldots.29\%\ldots\ldots\ldots\ldots}$$

$$\text{Moos}\quad \frac{\text{NFT}..8\%..\text{FT}..5\%..\text{F}}{\ldots\ldots\ldots.20\%\ldots\ldots.}$$

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[38] Frank's FTi minus NFTi score difference (and critical ratio) is practically unaltered by selecting the last instead of first 500 trials (cf. table 18, footnote †). The other two groups compared here are the entire series for both animals.

A similar arrangement might be made of the corresponding critical ratios of these differences. It should be noted in table 18 that the mean accuracy for both incentives is relatively constant from one series to another; this fact is important in any such comparison of differentiation scores in delayed response.[39] This level of performance was experimentally controlled by careful adjustment of the imposed delays from session to session.

If we grant that there was little or no general change in the animal's capacity for delayed response throughout these series of trials,[40] and if, as was the case, the mean per cent correct choices remained fairly constant, then the experimentally varied delays should be a function primarily of the changes in concurrent incentives from series to series. The last three series in table 18 (tokens immediately exchangeable) shall be considered.[41] In comparing

TABLE 18

Differential delayed response for food-token, non-food-token, or food incentives

Groups of consecutive trials, equal in number, for comparison of quantitative differentiation of incentives. FT = food-token; NFT = non-food-token; F = food; i = immediate exchange; d = delayed exchange.

INCENTIVES COMPARED	FRANK					MOOS				
	N	Mean delay	Per cent correct	Mean difference	C.R.	N	Mean delay	Per cent correct	Mean difference	C.R.
		seconds		*per cent*			*seconds*		*per cent*	
FTd vs. NFTd*	500	10.6	81.8	15.6	4.6	300	5.8	73.0	3.3	0.65
FTi vs. NFTi†	500	23.6	88.2	21.2	7.8	300	13.7	88.0	8.0	2.2
F vs. NFTi	500	21.1	83.1	29.2	9.4	300	24.5	85.3	20.0	5.1
F vs. FTi	500	42.0	86.4	1.6	0.52	300	26.4	87.7	4.7	1.2

* Actual trials nos. 141–640 for Frank, trials 341–640 for Moos, from their 640-trial series.

† Actual trials nos. 1–500 from Frank's 560-trial series. For trials 61–560, per cent correct = 87.8, mean difference = 22.0, C. R. = 8.0.

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[39] For example, too high a mean level limits the score of the preferred incentive, thus reducing differential scores.

[40] See Nissen and Elder (1935, pp. 58–61).

[41] The first two series were compared in this respect under "Food-token versus non-food-token; immediate exchange" (p. 72).

each pair of these three series for either animal, if we assume that the type of incentive which is common to both series contributes equally in each to the delay which can be imposed, we may subtract the average delays in each case and arrive at the relative effect of the other two incentives upon this variable delay. With this understanding, we may say that, for the same average accuracy of choice by the animal, the experimentally imposed delays were equal or greater when the incentives were direct food than when the incentives were food-tokens (comparing the second and third series). Likewise, for the same average accuracy of choice, the imposed delays were greater when the incentives were food-tokens than when the incentives were non-food-tokens (comparing the third and fourth series). Finally, for the same average accuracy of choice, the imposed delays were much greater when the incentives were direct food than when the incentives were non-food-tokens (comparing second and fourth series). Hence this measure, the imposable delay for constant accuracy, substantiates in direction of difference, if not in amount of difference, the relative efficacy of these three incentives as found above by comparing score differences within each series.

In order to study the distribution of errors within the experimental session, the data of table 19 were arranged. These selected groups of trials were the only available ones during which the imposed delay was constant for any considerable number of trials. It may be seen that in every case appears the tendency for higher accuracy, or fewer errors, during the latter half of any session. This difference is fairly reliable for Frank, consistent but hardly reliable for Moos; and in either case, this increase in accuracy from first half to last half of the session is a similar characteristic of both the delayed exchange session and the immediate exchange session.

H. Preference tests for all incentives

The results of these preference tests can be stated very briefly. Frank and Moos were given paired, pull-in presentations, with series of either immediate or delayed exchange of the tokens, of the following incentives arranged for successive single choices:

food *vs.* non-food-token, food *vs.* food-token, and food-token *vs.* non-food-token. Series of choices from one pair only, as well as series of all three pairs mixed in random order, were given. Bimba was presented pairs only of food *vs.* food-token. In only one single choice of these 596 presentations to all three subjects was the second named member of any of the above pairs chosen.[42] This lone exception was Moos' choice of a food-token with immediate exchange in preference to the food itself. This occurred during a series of 20 paired presentations of these two incentives.[43]

TABLE 19

Differential delayed response

Distribution of errors during experimental session when imposed delay is constant, showing mean per cent correct for first half-session and second half-session during consecutive series of trials.

INCENTIVES	ANIMAL	DELAY	TRIALS	TOTAL N	PER CENT CORRECT		DIFFERENCE 2ND–1ST	$Diff./\sigma_{diff.}$
					1st half	2nd half		
		seconds					*per cent*	
FTd vs. NFTd	Frank	10	161–400	240	78.3	87.5	9.2	1.91
	Moos	5	281–380	100	74.0	78.0	4.0	0.47
F vs. NFTi	Frank	20	161–460	300	81.3	87.3	6.0	1.44
	Moos	20	141–200	60	83.3	86.3	3.3	0.36

These preference tests accomplished two purposes: the previously trained preference for food-token to non-food-token was found intact at the completion of the last problem; more important was the verification in simple, relative preferences of the comparative reinforcing strengths of those same incentives which were employed in the delayed response problem.

V. CONCLUSIONS AND DISCUSSION

The major results indicated by the foregoing data can be summarized as follows:

1. It was verified that chimpanzees will perform work for discs exchangeable for food. They will work not only for single tokens,

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[42] For distribution of these 596 preference trials for each animal, see procedures (pp. 28–29).

[43] These 19 out of 20 choices in which the food was preferred represent a difference from chance with a critical ratio of 3.7.

immediately exchangeable; but, after certain training, they will readily work for ten to thirty tokens, exchangeable as a group after obtaining them singly. Individual differences, especially in emotional functions, seem to play a large part in determining the practical limit of such a group.

2. It has been established that food-tokens, with grouped exchange for food spatially and temporally remote from the learning-apparatus, were the adequate differential incentives for learning and retention by chimpanzees of the following problems: (a) Simple left- or right-position habits; (b) complex five-choice position habits; (c) visual size discrimination; (d) visual color-pattern discrimination; (e) delayed response. In problems (a), (b), (c), and (d), learning was evident from the fact that all subjects met certain criteria of mastery: response to the food-token baited stimulus (hereafter called "correct response") during the criterion trials with a frequency significantly in excess of chance expectation.

Other common characteristics of this learning were: progressive increase in the frequency of correct responses, from a level near or below chance expectation when differential rewarding began, until the criterion was met; increase in frequency of correct responses during single experimental sessions, prior to exchanging tokens and receiving food; general progressive increase in rate of response from first half to last half of habits; general high day-to-day motivation.

Food-tokens were adequate incentives for accurate delayed responses after imposed delay intervals as great as 120 seconds.

Each of the five problems listed above was mastered by at least one animal who had not previously received food directly in the learning apparatus or even in the room in which the apparatus was located.[44] In other words, it has been demonstrated that each of the problems was learned by a chimpanzee when the only object in the problem situation directly associated with food-reward was the food-token. Furthermore, entire habits, whether requiring but a single trial as the simple manipulation of the work- or learning-apparatus, or whether requiring up to 20 trials as certain position habits, may be completely learned in one session with food-tokens as the sole differential reinforcing agents, prior to any reception of food reward.

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[44] See chronological sequences of training as given in table 2 (p. 11). For Frank and Moos, this observation on the previous absence of food does not apply, of course, *after* the first habit with token incentives in the five-position learning, which for these animals, but not for Bimba, was followed by a direct food rewarded habit.

Evidence of retention of solutions to problems (a), (b), (c), and (d) was shown in the definite perseveration of such habits when the alternative stimuli were being rewarded during the early trials of a reversal of that habit.

3. It was verified that with little training chimpanzees would respond differentially to food-tokens and non-food-tokens; the former being exchangeable for food, the latter being differently colored and not exchangeable for food. This difference in response was first evident from the animals' higher rate of work and greater maximum number worked for in the case of the food-tokens.

With extended training, selection of the food-token in preference to the non-food-token became more frequent, first when immediate exchange was allowed, and finally when only grouped exchange was allowed.

Subsequent preference tests, with grouped exchange of tokens, demonstrated that this preference for food-tokens persisted without any loss over relatively long periods of time.

After the preliminary differentiation training, general behavior gave evidence that neither type of token had any significant play- or activity-value.

4. The incentive- or reward-value of the food-token was partially ascertained by comparisons with the *non-food-token* in: (a) Visual color-pattern discrimination (grouped exchange of tokens); (b) delayed response (immediate exchange of tokens); (c) simple preference tests (immediate or grouped exchange).

In problem (a), frequency of response to the food-token rewarded visual stimulus rapidly increased until a criterion of mastery of that habit was reached, although all responses to the alternative stimulus were "rewarded" with the non-food-token. In number of trials to criterion, a habit so rewarded was learned neither more nor less rapidly than one rewarded with food-tokens only. Neither did rate of response show that one method of reward was superior to the other. Nevertheless, the accuracy of all responses, taken together, demonstrated that the latter method of rewarding (food-tokens only) led to a significantly greater per cent choice of the food-token baited stimulus than did the method of food-token and non-food-token.

In problem (b), the food-token was accompanied by much higher total accuracy of responses (after delay) than was the non-food-token, the difference being consistently large and significantly in favor of the food-token. There is indirect evidence that, for the same average accuracy of choice by the animal, the experimentally imposed delays were greater when the incentives were food-tokens than when the incentives were non-food-tokens. While the subjects continued not to "exchange" the non-food-token when given the opportunity, their scores on non-food-token trials remained well above a chance level.

The simple preference tests (c) showed that the food-token was always preferred to the non-food-token, whether exchanged (or deposited) immediately or later as a group.

5. The incentive value of the food-token was further ascertained by comparisons with *direct food reward* in: (a) The five-position problem (grouped exchange of tokens); (b) the delayed response (immediate exchange of tokens); (c) simple preference tests (immediate or grouped exchange of tokens).

In problem (a), the trials required to meet a criterion of mastery were generally less (Frank) or equal (Moos) for the food-rewarded habits; but the frequency of correct responses, as well as rate of all responses, definitely indicated the superiority of the direct food incentives. When the habits were rewarded for equal periods, direct food was significantly more effective than the food-token, as measured both by rate and correctness of responses. Observations of general behavior indicated a lower level of general motivation with food-tokens than with direct food.

In problem (b), direct food was accompanied by only slightly higher total accuracy of responses (after delay) than was the food-token, the difference not being consistently in favor of food. This difference was also much smaller and less reliable than in the case of food-token *versus* non-food-token (immediate exchange of food-token). There is indirect evidence that, for the same average accuracy of choice by the animal, the imposed delays were equal or greater when the incentives were direct food than when the incentives were food-tokens. General motivation was higher during the food *vs.* food-token comparison than food-token *vs.* non-food-token, as measured by ratio of mean imposed delay to mean accuracy score; but two times as much food was actually received in the food *vs.* food-token comparison. General behavior reflected this difference in general motivation.

The simple preference tests (c) demonstrated that direct food was always preferred to the food-token, whether exchanged immediately or as a group.

6. As complement to the above comparisons of food-token with direct food and non-food-token, a comparison of direct food with non-food-token was made in: (a) the delayed response, and (b) simple preference tests.

In problem (a), the difference between per cent correct responses for food incentives and non-food-tokens was greater for each animal than a sum of the differences recorded for food *vs.* food-token and food-token *vs.* non-food-token. Reliability of the present difference was consistently higher than reliabilities of the difference in either of the other two comparisons. As in the comparison, food-token *vs.* non-food-token, it also was apparent that the responses to the non-food-token were correct in far more cases than would be expected by a chance distribution. There is indirect evidence that, for the same average accuracy of choice by the animal, the imposed delays were much greater when the incentives were direct food than when the incentives were non-food-tokens. While general motivation remained high, measured by such indices as distractibility or willingness to respond during delayed response trials, the animals became extremely "negative" toward depositing the non-food-token.

The simple preference tests (c) demonstrated an unremitting preference for direct food whenever paired with the non-food-token.

7. In comparing all three incentives it was shown, on the basis of quantitative comparisons made in the delayed response problem,[45] that they could be arranged roughly on a linear scale for each animal, representing relative efficacy as incentives for correct responses in that problem. For either subject, direct food is *most* efficacious, the non-food-token *least* efficacious (although considerably above zero efficacy), and the food-token *intermediate* in efficacy (although much nearer the value of food than the value of the non-food-token). Data from the food *vs.* food-token comparison in the five-position problem, as well as data from the food-token *vs.* non-food-token comparison in the visual color-pattern discrimination problem, all substantiate these relative values of the incentives.

A final corroboration of these comparisons in terms of simple paired preferences was carried out, employing presentation of mixed series of pairs. These included all three combinations of two incentives from food, food-token, and non-food-token. Results showed a persistent and clear-cut preference for food whenever paired with the food-token or the non-food-token, and for the food-token whenever paired with the non-food-token.

8. It was further shown in two series of delayed response trials that there was a marked rise in the mean accuracy of response in that problem when the food-token incentives could be exchanged immediately, as compared to exchange only after a group had been collected. The precise effect upon the difference in accuracy for concurrent trials with either food-token or non-food-token incentives could not be determined from the data at hand.

From the results summarized above it may be concluded that the chimpanzee is readily trained not only to work for tokens exchangeable for food, but to work for several of these food-tokens before exchange is allowed. It is further evident that these tokens have operated as incentives for the acquisition of a variety of complex habits, in situations temporally and spatially removed from the final food reward. It was also shown that learning and retention were only slightly less efficient with the food-token than with immediate food-reward, yet the food-token was far more efficient than a token for which the animal never received food (non-food-token). Finally, there was evidence that when the point of exchange of food-token was spatially and temporally closer to the problem situation, the efficacy of the token as an incentive in delayed responses was heightened.

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[45] Employing immediate rather than grouped exchange, or deposit, of tokens.

These results suggest certain possible explanations of the mechanisms involved in the observed behavior. In seeking to describe the essential nature of the token-reward, at least in eliciting previously acquired responses, Wolfe (1936, p. 70) makes the following statements:

> There are a number of ways of characterizing a token (poker chip) which has through use become associated with a primary reward. It may be considered as: (1) a sign or symbol of something to come or happen, e.g., food, water, or an activity-privilege; (2) an instrument, a tool, a means to an end, a manipulandum; (3) something which, because of certain past associations, now has intrinsic value. One is unable to choose between these possibilities from the present experiments. To regard a token as a sign or symbol (1) seems justifiable provided a distinction is made between a physical object and a language process or a pure stimulus act. But symbolism has so many different connotations that its use is confusing. The second and third characterizations seem preferable.

To discover the basic mechanisms here, it may be fruitful to relate the situation of the token-reward to similar aspects of certain other experiments in which the chimpanzee has been a subject.

The simple act of working for a token and immediately exchanging it for food seems not to be qualitatively different from certain instances of behavior variously reported by those working on "instrumentation," "tool-using," and the like. We would include here those cases in which the animal manipulates an object (alters the environment) in such a way that the necessary connection between animal and goal is effected. This may take the very simple form of depressing a lever which releases the food, or opening a door to gain access to the food. There is a minimum of locomotion of the animal; and the connection between the object manipulated and the goal is not only direct, but subject to little variation on the part of the animal. In fact, experimental situations with these characteristics have not been limited to the primates. A rat, for example, can be taught to depress a bar which automatically delivers previously concealed food; this learning may be effected by baiting the end of the bar directly with a lure. When the bar is accidentally moved, more food appears; and as long as the rat is hungry he will continue to perform this habit.[46] The bar itself then appears to be an "incentive" for each repetition of the act; for, in each case, until the bar is moved no food is directly present to the animal.[47] However, we can distinguish between this sort of an incentive, which is fixed in one situation and elicits a single, previously learned act, and the incentive which elicits more varied behavior.

In more complex situations the manipulable object must be carried by the animal, or moved into a position not predetermined by limits in the object's motion. This is characteristic of typical experiments on instrumentation of primates. In these experiments interest has generally been focused on the animal's initial solution of the perceptual-motor problem involved, rather than on its utilizing the behavior sequence in subsequent performance of the same act. In some cases, however, the new situation has had many features in common with preceding situations, which differed only slightly one from the next.[48] The chimpanzees of Bingham (1929) and others used boxes as the intermediary in various suspended-food situations.

Now, whether we speak of the manipulated object as a "sym-

[46] This general technique is illustrated by Skinner (1932) with rat subjects: depressing a bar releases a food-pellet. Analogous would be the technique of Klüver (1933) with monkeys: pulling a string secures food from out of reach.

[47] In the same sense, we might extend "incentive" to the wooden cover of the baited container in our position-habit experiment, or to the stimulus plate baited in the discrimination experiments (assuming that in each case the choosing-habit was already learned).

[48] Further evidence could be cited from the work of Guillaume and Meyerson (1934) or from Jackson's unpublished studies on instrumentation. See Jackson (1933, 1934) for abstracts of preliminary reports.

bol," "instrument," or "sub-goal"[49] seems dependent on our assumptions as to the nature of the behavioral interconnection effected by this object between animal and final goal. In the opinion of the writer, to the manipulated object can be attributed two sorts of function,[50] differing perhaps only in degree. In the situations to which we have referred above, those involving the repetition of a single, fairly uniform sequence (depressing the lever time and again, or using a token in a repetitive situation), we need only assume that the object is merely a necessary part of a relatively unvarying sequence. We need not assume that the object has even temporary "intrinsic" value, any more than has any other object responded to in that sequence. At best, it is a *sub-goal only for the same, practiced act* upon each occasion for running off the behavior sequence, as lifting a weight, in the sequence of obtaining a token and then exchanging it for food. It has become so associated and identified with the very same sequence (just as has the lever in obtaining a food-pellet for the rat) that we cannot say anything of the relative independence of that object as a goal, as distinguished from other objects in the same situation. It is as gratuitous to call a stick or token a secondary reward in such situations, as it is to call the lever a secondary reward—unless the mere difference in portability be a justifiable criterion.[51]

Moreover, the fact that the animal will perform the necessary work-operations for the object (e.g. token) several times in succession before the final goal is attained cannot tell us much more of the intrinsic incentive- or reward-value of the food-token,

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[49] This term is used to denote Wolfe's third alternative, "something which, because of certain past associations, now has intrinsic value," (1936, p. 70).

[50] Wolfe (1936, p. 2) has mentioned this difference but has applied "secondary reward" to both functions:

"... under certain conditions the elicitation of a previously established sequence of acts by means of an object associated with a primary reward is evidence that the object is a secondary reward. If an object, for example, which has previously been associated with food elicits a series of acts which have never before led to the object but have led to food, then the food-associated object has been established as a secondary reward."

[51] And likewise, we would be obliged to consider the stylus of a stylus maze, or even the electric key in a reaction-time experiment, as "secondary rewards."

unless it be shown that we are not dealing with a relatively simple "reflex" sequence which obeys the laws of progressive extinction during the series of unreinforced (unrewarded) acts. Wolfe has shown, for example, that the animals perform in the token situation with a negative gradient similar to the progressive falling off of responses of rats in the Skinner type of situation.[52] He also found that such an extinction series was similar to the negative gradient of responses to lifting a weight with no rewards at all until food was given at the end of ten minutes.[53] In short, if the token has acquired some temporary "intrinsic" reward-value, the responses leading to it should show resistance to extinction after removal of the primary reward (food). Tolman (1937) has expressed this in terms of "secondary demands;" and his very appropriate usage of "fixations" indicates a limiting case in permanence of a secondary demand. The food-tokens may, aside from any association with the food-goal, possess intrinsic "fixation" of reward-value as an object of play, as something to be mouthed, and so on.

This brings us to the other type (or degree) of function of the manipulated object, distinguishable from the work-producing one we have just discussed: that is, its effectiveness as a relatively independent sub-goal, or secondary reward. By this we mean its effectiveness in eliciting not one previously learned behavior sequence, as lifting the weight or depressing the lever, but various different behavior sequences *not previously associated with it.* When the chimpanzee searches for a key to a familiar lock,[54] or shapes a board into the form of a stick which previously was available as the usual "tool,"[55] or seeks and tears a bar from a "shoe-scraper" to use in place of the missing familiar stick,[56] or even when he goes about moving *for the first time* the weight-lifting device in order to secure the token which customarily has been the necessary part of food-getting[57]—in these cases we must admit that the "new" behavior is brought about, or directed,

[52] Wolfe (1936, p. 37, table 11).

[53] Wolfe (ibid., p. 36, table 10).

[54] Yerkes and Yerkes (1929, p. 363).

[55] Bingham (1929, p. 5).

[56] Köhler (1925, p. 105).

[57] Wolfe (1936, p. 17).

by the manipulable object (key, stick, or token) as if it had intrinsic incentive value. We are justified in calling the object a "sub-goal" or "secondary reward" if, and only if, its apparent value is the result of specific previous associations with a primary goal or reward. Moreover, we can demonstrate that the object functions as a sub-goal rather than as one of any number of stimulus features in the situation, all with but slight incentive value,[58] only by testing whether that object by itself is adequate for the eliciting and associating of behavior not previously connected with it. It is characteristic of a final, or primary, goal (food) that it will elicit a multitude of behavior; other objects, encountered on the route to the final goal, elicit behavior not part of that sequence only when they have become firmly associated with that goal, thereby functioning as sub-goals. Furthermore, how far such objects may be behaviorally separated from the final goal and still operate as incentives to new behavior seems to be related to the strength of association with the final goal.

The major part of this investigation was devoted to an examination of just this latter function of the food-token: its effectiveness in eliciting and integrating behavior not previously associated with it. Our experiments have shown varying degrees of complexity of behavior which can be thus integrated. The positional-choice problems are fundamentally similar to those casual observations on the chimpanzee's search for a familiar tool; we have shown, however, that the animal will form new habits where the position of the token occupies different positions in turn, and that measurable improvement in economy of the searching behavior will occur not only from habit to habit but especially within that period before the token becomes the "tool" for obtaining the food-goal. This last fact is important in indicating the relative independence, or temporary "intrinsic" value, of the token. It does not, in these situations, exhibit the phenomenon of rapid extinction characteristic of responses to an object, which is merely a part of a repetitive behavior sequence, from which the final goal has been removed. Similarly, we have shown that the food-token will operate adequately as incentive for the acquisition of other complex habits: visual discriminations, and the retention of critical responses in the delayed response problem. These last two are especially remarkable in view of the fact that we utilized a technique of immediate second choice and reward in case of error: the relative economy to the animal in attaining the *final goal* was certainly less than discriminable in the event of correct choice as compared to error choice. If, on the other hand, the token is considered the immediate goal (or a sub-goal), this economy is much greater in proportion, and the rapid learning of the animals to make correct choices is then less remarkable.

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[58] In this connection see above, in Introduction, p. 5, also footnote 3.

Those parts of our investigation in which the effectiveness of the token was compared to that of direct food-reward, or of the non-food-token, serve as controls which emphasize the interpretations we have drawn above. Wolfe has shown that a non-food-token is relatively ineffectual in eliciting a previously established motor sequence; our own experiments show that the non-food-token is likewise relatively ineffectual in eliciting responses not previously associated with the token.[59] Nevertheless, as the data on delayed responses indicated, we have seen that the non-food-token may have acquired certain intrinsic goal value, not from any direct association with food (as exchange for food) but rather perhaps by association with (or irradiation from) the food-token itself. In any case, we have adequately demonstrated that the essential reinforcing- or goal-value of the food-token is dependent upon its association with the final food goal. The comparisons with direct food reward indicate that the token-rewards for the acquisition of behavior patterns are only slightly less effective than the former type of reward,[60] while the non-food-token is far less effective, if effective at all.

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[59] By using a brass disc, instead of a differently colored token of same size and material as here, Wolfe allowed the animal additional cues for discriminating the non-food-token from other tokens, as well as possible bases for significant intrinsic value differences for the animal: for example, kinesthetic (weight), auditory (sound when dropped or manipulated), or tactual (metallic *vs.* resin textures). These facts do not materially affect our interpretations. (Cf. Wolfe, 1936, pp. 13–16).

[60] Compare the illuminating observations of Nissen and Crawford (1936, p. 401 ff.) on the relative efficacy of food and tokens in eliciting certain food-sharing behavior.

Before any special theoretical system of explanation can be applied to these facts, further relationships between the variables we have studied must first be determined. Analogies between functions of the food-token and conditioned response principles, especially higher order conditioning, are readily drawn. Exchanging the food-tokens for food may be looked upon as a reinforcing process, in which number of reinforcements of the conditioned stimulus (token) is a basic variable in determining its strength. The food-conditioned stimulus becomes in effect a kind of "secondary conditioned stimulus," which, if reinforced at intervals to prevent experimental extinction, can serve as the basis for further, higher-order conditioning (as the reinforcing of new habits in our experiments above).

This analogy cannot be pressed too far, for the strength of the food-token seems clearly less subject to extinction than that of a secondary conditioned stimulus in the usual conditioning experiment. Reasons for this difference may be that (1) the token, when possessed by the animal, is a sustained external stimulus;[61] (2) more complex or more integrated segments of behavior are involved in the token situation than in the more nearly "reflex" type of behavior in usual conditioning; (3) the token is a discrete *object* (not an *act*) which is free to be carried about, and also other activities may occur simultaneously without interfering with the maintenance of responses toward the token; (4) the token is something which the animal must "give up," or exchange, while in the conditioning situation the animal is more or less passively acted upon by certain imposed stimuli. Any more analytical description of these differences seems, at present, more suggestive than conclusive.

Neither this nor Wolfe's experiment varied to any extent the behavioral separation of final goal and the situation in which the animal received the tokens. In both cases the food was out of sight until the tokens were exchanged; there are also the data on the greater effectiveness of tokens immediately exchangeable as compared to those exchangeable only after a group had been obtained, both in the work- and in the learning-situations. In the present experiment, the effect of spatio-temporal separation was observed, the indication being that greater immediacy of exchange, spatial or temporal, seems to result in greater efficacy of the token-incentive. In any event, there is abundant evidence that after certain training the final goal may be widely separated from the situation and yet permit the food-token to be highly effective in eliciting behavior not previously associated with it.[62]

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[61] Finch and Culler (1934) have shown how a heightened general motivation may sustain higher-order conditioning: here the token might be thought of as combining these effects. Wolfe (1936, p. 68) suggests that the presence of the token may either reinstate the food-situation, maintain attention, or prevent the animal from discovering the delay before securing food.

Related to this question of behavioral separation of token and final goal, is the factor of number of tokens received before exchange is allowed. Wolfe has indicated that effectiveness of the later acquired tokens may depend upon the number of similar tokens already possessed by the animal. If this occurred during our learning sessions, the negative effect was counterbalanced by the general improvement in learning. Furthermore, neither investigation attempted to find the actual numerical limits of the collecting of tokens before exchange: the gradual increase in number from session to session, as shown in our results, was a function particularly of the animal's progressive increase in rate rather than experimental extension of the time limit. In the present study, the number was kept quite constant throughout training on the learning problems, without any attempt to determine the maximum feasible during learning sessions.

Wolfe has further shown that to the token may be assigned not only food-values but also activity values such as play or return to the home cage, and that such tokens are effective in eliciting established behavior patterns; how far such tokens may be effective in the learning of standard problems involving the acquisition of new habits is yet to be determined.

As Nissen and Elder (1935) and others have shown for quantitative differences in food-reward, the present investigation has also shown that differential scores in learning or retention can be induced by direct food, food-tokens, or non-food-tokens each compared with the other. It is yet to be demonstrated that tokens associated with quantitatively different final goals (one amount of food *vs.* twice that amount of food), or different quantities of tokens (one token per trial *vs.* two tokens per trial), will also affect learning differentially.

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[62] In the later portion of training, certain protocols show that food-tokens continue to be highly effective as incentives to learning, even though over an hour may elapse during the session prior to the exchange of tokens.

In brief summary of variables obviously needing further investigation before we can formulate definite laws of the mechanism of token-rewards as incentives for the learning of new habits we mention the following:

(1) Spatial or temporal separation of final goal and receiving of the token incentive.[63]
(2) Number, recency, and nature of previous reinforcements of the token by exchange for the final goal.
(3) Relative weight of general motivational factors (total amount of food reward, coöperation with experimenter, intensification of situation with noises, additional rewards, and so on) as against specific motivational factors (nature of goal associated with token, intrinsic play- or other value of token).
(4) Effects of one type of token upon the specific strength of another type of token in the same situation, (the tokens differing qualitatively or quantitatively).[64]
(5) Relation of difficulty of problem, or stage of learning attained, to strength of token incentive.

VI. SUMMARY

1. Chimpanzees readily performed work to obtain discs which were immediately exchangeable for food. They continued to work even when they were not allowed to make the exchange for food until after a group of tokens had been secured. With certain training, they consistently worked for groups of 10 to 30 tokens before exchange.

2. These food-tokens, after the above training (1), were presented as incentives in standard learning problems, in a separate

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[63] Here must be included the factor of number of tokens, discussed above.

[64] Compare Nissen and Elder (1935), who pointed out "perseverative" effect of different food-incentives.

room from that in which the tokens were exchanged for food. Exchange for food was allowed only after a group of 10 to 36 learning trials had been conducted. Under these conditions the tokens were adequate incentives for the acquisition and retention by these animals of numerous habits comprising the following types: simple left- or right-position habits, complex five-choice position habits, visual size- and color-pattern discriminations, and delayed response. Entire habits requiring up to 20 trials, as certain position habits, may be completely learned in one session with food-tokens as the sole differential reinforcing agents, prior to any reception of food reward.

3. The animals readily learned to differentiate preferentially between these food-tokens and differently colored tokens which yielded no food upon exchange.

4. Comparisons of their strengths as incentives for the acquisition and retention of responses showed that the food-token was slightly less efficacious than the directly received food and much more efficacious than the tokens which yielded no food upon exchange.

5. There is considerable evidence that the food-token was an adequate incentive for learning and retention only by virtue of its exchange for food.

6. There is evidence that food-tokens immediately exchangeable are more effective incentives for learning and retention than food-tokens exchangeable only after a group has been collected during successive trials.

7. Preference tests and observations of general behavior of the animals substantiated the relative strengths of these types of incentives (food, food-token, non-food-token) as determined in the problem situations.

8. Certain analogies are apparent in comparing the functions of the food-token with those of any acts or stimuli instrumental in, or associated with, the attainment of a primary goal in repeated performances of a previously established activity. Higher order conditioning furnishes additional points of similarity. The uniqueness of the token as used in this investigation seems to lie particularly in its object-characteristics, its manipulability, its

potentiality as a source of maintained external stimulation, and in the fact that it must be given up or exchanged for a primary goal. These characteristics and conditions apparently give to the food-token, as we have used it, great effectiveness in the learning of new habits and unusual resistance to "experimental extinction" as compared to instrumental acts and primary or higher orders of conditioned stimuli. These differences, it is suggested, are in degree rather than in kind.

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THE INTERRELATIONSHIP OF DRIVES IN THE MALE ALBINO RAT

II. INTERCORRELATIONS BETWEEN 47 MEASURES OF DRIVES AND OF LEARNING[1]

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I. INTRODUCTION

In an earlier study (2), the writer obtained data concerning the interrelationship of a number of measures of drives. In that study the different drives were measured by a comparatively limited number and variety of tests, and it was therefore difficult to separate the possible influence of a common drive factor from the possible influence of the general similarity of the test situations. Furthermore, the various tests were given with but a small amount of preliminary adaptation upon the given apparatus and, as was pointed out in the previous report, it is therefore probable that emotional and exploratory factors affected the animals' performance upon any given test and thus may have produced correlations which would not otherwise be obtained. The earlier study was also deficient in that adequate reliability measures were not obtained for some of the tests.

The present study was therefore undertaken to obtain more adequate and comprehensive data concerning the interrelationship of various measures of different drives and the relationship of the motivational measures to learning performance. The number of tests used was greatly increased, and each drive was measured by several tests, some of which differed markedly in character. In order to avoid, in so far as possible, the influence of extraneous factors which might affect the correlations between tests, the animals were given extensive preliminary adaptation upon the various pieces of apparatus before being tested. The general techniques of testing were also improved, and each test was broken up into short subtests in order that more adequate reliability measures could be obtained.

With the improved techniques, a total of 47 measures were obtained from a group of 51 male albino rats. Of the 47 tests given, 7 were measures of exploration, 14 were hunger tests, 10 were thirst tests, 7 were sex tests, 6 were learning scores, and one measure each was obtained for emotion, body weight, and general activity. The present paper is devoted to a description of the tests and the presentation of the intercorrelations between the 47 measures obtained.

The intercorrelations between different measures of the same drive will throw light on the problem as to whether or not a specific drive such as hunger, thirst, sex, or exploration, should be considered as a more or less unitary factor important in determining individual differences upon tests involving the given drive. The intercorrelations between measures of different drives will provide evidence concerning the dependence or independence of the specific drives and concerning the existence of a general drive factor. The study also supplies data concerning the interrelationship of drives, emotion, and learning performance; and concerning a number of less general problems.

II. PROCEDURE

The experimental procedure will be discussed under the following headings: (A) Animals and Pre-Experimental Handling; (B) Names and Order of Giving Tests; and (C) Apparatus and Tests.

A. Animals and Pre-Experimental Handling

The animals tested were 55 male albino rats, 125 days old (no age variation) at the time of the first test given. At this age their average weight was 277 grams and the range of weights was from 192 grams to 331 grams. The rats were raised on a modified Steenbock (10) diet, supplemented twice weekly with lettuce. The animals were used in a sex maturity study and in connection with that study were tested on alternate days for the first evidence of ability to copulate while in, or approaching, the adolescent period. These maturity tests were begun at the age of 35 days and were continued until each of the males had successfully passed them, the last animals doing so at 83 days of age. As the maturity tests were part of an independent study they will not be considered further in the present paper; their only function for the present study was to serve as a period of preliminary handling and of preliminary copulatory experience.

After passing the sexual maturity tests and until the age of 99 days all of the males were released in a large laboratory room with a floor space of about 16 square meters. When the males were 83 days old (all had passed the sexual maturity tests), 12 adult females were introduced into the room with the males to allow opportunity and freedom for sexual activity. Non-pregnant females were introduced weekly. At 99 days of age, the males were transferred in groups of 5 to laboratory cages (18 x 18 x 8 inches) in which there were two non-pregnant females. The females were replaced by two non-pregnant females weekly. The males were caged according to body weight so that all 5 males in any cage were of approximately the same weight. At 110 days of age the females were removed from the laboratory cages and one week later each male was allowed a preliminary 15-minute copulating period with a female in oestrum. If any male failed to copulate during this 15-minute period, he was given a series of opportunities to copulate extending over 25 days. The purpose of these auxiliary sex activation periods was to induce all rats to copulate, if possible, before they were given the regular copulation tests.

The procedure described above was the only preliminary training or experience given the rats before their regular series of tests began at the age of 125 days. Preliminary training for specific tests will be described in connection with the tests involved. During the course of experimentation a few of the original group of rats died. The size of the group involved will therefore be designated at the appropriate places. The results for most of the tests are based upon 51 rats of the original group of 55 animals.

B. Names and Order of Giving Tests

The order in which the tests were given and a brief summary characterization of each test are presented in table 1. Details concerning the apparatus and the testing techniques used will be presented in the section on apparatus and tests.

Names and Numbers of Tests: In general, a word or brief phrase indicating the particular apparatus involved in a specified test has been used to identify the particular test. The word or phrase descriptive of the apparatus has been selected upon the basis of brevity rather than of completeness of description. To further identify a specified test, a word indicating the particular motivation involved follows the apparatus word. Thus a test using the Columbia obstruction apparatus and given to hungry rats running to a food reward is described as an "obstruction hunger" test. Tests given in a simple observation cage or in the rat's own living cage have been identified by a word indicating the type of activity involved, e.g. eating tests, drinking tests, copulation tests.

Numbers have been assigned to the tests in accordance with the chronological position of the test in the testing schedule. The test number thus gives an indication of the relative position of any particular test in the program.

Use of Subtests: From earlier work of the writer (2) it appeared probable that tests could be made more reliable if they were given in short subtests rather than as one continuous test. All of the tests of the present study have therefore been divided into short subtests. The length of the subtests of the different tests used varied as indicated in table 1. In general, the subtests were made long enough to permit an adequate range of scores and were made short enough to allow the entire group of animals to complete one subtest in a day's testing. Tests in-

TABLE 1

Testing program—giving the number and name of each test, the apparatus and motivation involved, and details concerning subtests, age at testing, and body weight

TEST NUMBER	NAME OF TEST	APPARATUS	MOTIVATION	SUBTEST LENGTH	NUMBER OF SUB-TESTS	AGE IN DAYS AT SUBTEST				MEAN BODY WEIGHT AT 1ST SUBTEST
						1	2	2	4	
1	Dashiell maze exploration	Dashiell maze	Normal*	4 min.	4	125	127	133	138	277
2	Vertical maze exploration	Vertical maze	Normal	4 min.	4	131	136	141	145	
3	Open field exploration	Open field	Normal	3 min.	4	143	147	153	159	281
4	Emotional defecation	As in tests 1, 2 and 3	Normal			As in tests 1, 2, 3				
5	Copulation	Living cage	Sex	15 min.	4	148	152	156	160	291
6	Sex barrier time	Sex barrier box	Sex	4 min.	4	164	167	170	173	296
7	Sex barrier distance									
	Placed on restricted food diet					179				302
8	Open field food eating	Open field (food can present)	Hunger	3 min.	4	181	182	183	184	263
9	Open field food exploration									
10	Open field food barrier	Open field (with barrier)	Hunger	3 min.	4	185	186	187	188	248
11	Open field barrier exploration									
12	Jump hunger	Jump obstruction apparatus	Hunger	4 min.	4	192	193	194	195	233
13	Obstruction no shock hunger	Columbia obstruction apparatus	Hunger	4 min.	4	197	198	199	200	237
	Rest on full diet					200 to 204				
	Placed on restricted water diet					204				287
14	Drinking amount	Eating and drinking cage	Thirst	4 min.	4	206	207	213	214	257
15	Drinking amount per gm. body wt.									
16	Drinking time									

17	Salt drinking amount	Eating and drinking cage	Thirst	4 min.	4	209	210	211	212	259
18	Salt drinking time									
19	Jump thirst	Jump obstruction apparatus	Thirst	4 min.	4	217	218	219	220	256
	Rest on full diet					220 to 224				
	Placed on restricted food diet					224				297
20	Eating amount	Eating and drinking cage	Hunger	4 min.	4	226	227	228	229	275
21	Eating amount per gm. body wt.									
22	Quinine eating amount	Eating and drinking cage	Hunger	4 min.	4	231	232	233	234	255
23	Quinine eating time									
24	Sand hunger digging time	Sand apparatus	Hunger	1 Trial	4	236	237	238	239	253
25	Sand hunger idle time									
26	Sand hunger total time									
27	Speed hunger fast trials	Speed runway	Hunger	5 Trials	4	241	242	243	244	254
28	Speed hunger slow trials									
29	Obstruction hunger	Columbia obstruction apparatus	Hunger	4 min.	4	247	248	249	250	252
30	U-maze A errors	U-maze, pattern A	Hunger	5 Trials	6	253 to 258				246
31	U-maze A time									
	Rest on full diet					258 to 267				
	Placed on restricted water diet					267				295
32	Sand thirst	Sand apparatus	Thirst	1 Trial	4	270	270	271	271	259
33	Speed thirst	Speed runway	Thirst	5 Trials	4	272	273	274	275	258
34	Obstruction thirst, strong shock	Columbia obstruction apparatus	Thirst	4 min.	3	277	278	279		251
35	Obstruction thirst	Columbia obstruction apparatus	Thirst	4 min.	3	283	284	285		261
	Rest on full diet					285 to 287				
36	Copulation retest	Living cage	Sex	15 min.	3	287	296	314		

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* "Normal" means that the animals were taken from their living cage containing unrestricted food and water supplies and tested immediately.

TABLE 1—*Concluded*

TEST NUMBER	NAME OF TEST	APPARATUS	MOTIVATION	SUBTEST LENGTH	NUMBER OF SUB-TESTS	AGE IN DAYS AT SUBTEST				MEAN BODY WEIGHT AT 1ST SUBTEST
						1	2	3	4	
37	Vaginal plugs	Living cage	Sex	15 min.	7	As in tests 5 and 36				
38	Sand sex	Sand apparatus	Sex	1 Trial	4	289	292	293	294	297
39	Speed sex	Speed runway	Sex	5 Trials	3	299	301	304		302
40	Obstruction sex	Columbia obstruction apparatus	Sex	4 min.	3	307	309	311		306
41	Obstruction exploration	Columbia obstruction apparatus	Normal	4 min.	2	316	318			
	Placed on restricted food diet					319				314
42	Light discrimination, errors	Stone multiple light disc. box	Hunger	See note 1		322 to 327				288
43	Light discrimination, time									
44	U-maze B, errors	U-maze, pattern B	Hunger	See note 1		328 to 330				236
45	U-maze B, time									
46	Body weight					179	204	224	267	
	Rest on full diet					330 to 361				
47	Activity	Slonaker activity drums		See note 2		361 to 417				325

Note 1. See description of tests for details.
Note 2. Eight weeks in the drums, the activity score being based on the last four weeks.

volving hunger or thirst motivation were divided into subtests which were given daily, while the subtests of exploration and sex behavior were usually separated by an interval of several days. The number of subtests for any given test was usually four.

Preliminary Adaptation to Apparatus: In order to increase the reliability and validity of the tests used, much care was exercised in adapting the animals to apparatus and techniques by means of extended preliminary training. In tests of exploration, however, no preliminary adaptation was given as the rats' early responses are the point of interest. In order to conserve time in the testing program, preliminary adaptation to one apparatus was given while the animals were being given some other test. For example, while the animals were being tested for their speed of running to a food reward, they were also given a five minute preliminary feeding period in the obstruction apparatus at the conclusion of the day's speed trials. Thus, at the conclusion of the speed tests, the animals had had several days of preliminary feeding in the obstruction apparatus, which was the next apparatus to be used. Details of preliminary orientation training will be presented in the section on apparatus and tests.

Control of Motivation: Motivation was controlled in all cases of food or water deprivation by allowing the animals access to food or water daily for a limited time period only. The mean body weights presented in the last column of table 1 give an indication of the strength of the motivation at the different tests. The length of the daily feeding or drinking period was made such that the animals lost weight rapidly at first and then more gradually. When the animals as a group appeared to be working well, the reward period was made long enough to maintain the rats at a constant body weight. Recuperation periods were introduced at the points indicated in the schedule to prevent too great a body weight loss and to avoid systematic errors which might be involved in keeping the animals on reduced diets for too great a time period. The sex drive was controlled by keeping the males segregated and allowing them access to a female only in the test period or in the preliminary activation preceding the test.

Total Period of Testing: As indicated in table 1, the animals were tested almost daily from the age of 125 days to the age of 330 days. The activity test, test 47, extended over several weeks more and hence the rats were not entirely through the testing program until the age of 417 days. All rats were given the same test on the same day and at the same age, with a few exceptions in the case of some of the sex tests which required two days to give to the entire group of animals.

Hours of Testing: All tests were given between the hours of one and seven P.M. with the exception of all tests involving sexual motivation, and the light discrimination and maze tests. The sex tests were given at night between the hours of 8 P.M. and 1 A.M. Light discrimination and maze tests required longer to give than other tests and hence the hours for these tests extended from 1 P.M. to 10 P.M. usually, but occasionally to several hours later.

Position of Tests in the Schedule: The position of a given test in the program was partly determined by the availability of certain apparatus at a given time and by incidental considerations. Apart from such factors an effort was made to distribute tests through the program so that all obstruction tests, for example, would not be given within a few days. Tests of weaker drives, such as sex, were placed later in the program than hunger and thirst tests involving the same apparatus in order than the animals might have the advantage of greater familiarity with the apparatus at the time the weaker drive was tested.

C. Apparatus and Tests

To simplify the description of tests, the apparatus involved will first be described, then the tests given with that apparatus will be considered. Test numbers have been assigned in chronological order, hence a test with a high number was given late in the program.

1. *Modified Dashiell Maze.* This apparatus was similar to that described by Dashiell (3) but was larger. The blocks forming the sides of the alleys were 6″ x 6″ x 5$\frac{3}{4}$″ high. Forty-nine such blocks were used instead of the 16 small blocks of Dashiell's

maze. The width of the pathway between blocks was 4 inches. The entire maze was covered by ½ inch mesh wire. The maze was made larger than Dashiell's original design in order that its exploratory value might persist longer.

a. Technique. The animal to be tested was introduced into the maze through a swinging door in the middle block of one side of the apparatus. A stop-watch was started as soon as the animal entered the maze and a record of the animal's activity was taken for 4 minutes. At the end of the 4 minute period, the entire top covering of the apparatus was lifted and the animal was removed. Care was taken to avoid frightening the rat while he was being introduced to, or removed from, the maze.

b. Tests Using the Dashiell Maze. Only one test was given on this apparatus.

Test 1. Dashiell maze exploration. Four 4-minute subtests were given, the score being the sum of the scores on the subtests. The interval between subtests 1 and 2 was two days; between 2 and 3, six days; and between 3 and 4, five days. The test was scored in terms of the number of squares traversed, but the intersection squares were not counted.

2. *Vertical Exploratory Maze.* This apparatus was similar in general character to the Dashiell maze, but the animal was required to explore vertically by climbing from peg to peg instead of being required to explore by running through alleys made by the blocks of the Dashiell maze. The apparatus consisted of a vertical exploratory cavity 68″ high, 24″ wide, and 4″ deep, made by fitting together appropriate screens of screen wire. To make exploration possible, wooden pegs ¾ x ¾ x 4 inches were mounted between the front and back of the frame. The pegs were separated from each other by 4 inches. There were 13 rows of such pegs, and alternate rows were staggered to make progression easier for the rat.

a. Technique. The animal was placed in the apparatus on a small platform on the 7th row of pegs. He could then explore by hopping from peg to peg either across, up or down. At the end of the 4-minute test, one side of the apparatus was opened and the animal removed.

b. Tests using the vertical maze. Only one test was given on this apparatus.

Test 2. Vertical maze exploration. Four 4-minute subtests were given. The interval between the first and second subtests was 5 days; between the second and third, 5 days; and between the third and fourth, 4 days. The test was scored in terms of the number of pegs which the animal traversed. A peg was counted when the animal placed both hind feet upon it.

3. *Open Field Apparatus.* This apparatus was similar to the open field described by Hall (4). Three sheets of sheet metal 27½ inches high by 98½ inches long were bolted together so as to form a circle approximately 7½ to 8 feet in diameter. The floor was marked off in 9-inch squares to facilitate scoring.

a. Technique. Each animal was placed in the apparatus close to the outer metal wall and at the same point of the field. A record was kept of the animal's activity by counting the number of the nine-inch squares which he traversed. After 3 minutes the animal was removed from the apparatus.

b. Tests using the open field. This apparatus was used under three different conditions, namely: as a plain open field; with food present; and with a barrier surrounding the food.

The plain open field. One measure was obtained under this condition.

Test 3. Open field exploration. The animals, neither hungry nor thirsty, were given 4 subtests of 3 minutes each. The interval between the first and second subtests was 4 days, between the remaining subtests, 6 days. The score was in terms of the number of marked off squares traversed in the given time.

Open field food tests. For this test the animals were deprived of food and maintained on a reduced diet. A food can containing the animal's standard diet was placed in the center of the open field and the rats were introduced for four 3-minute subtests. The subtests were all separated by intervals of 24 hours. Two scores were obtained under these conditions:

Test 8. Open field food eating. This score consists of the time in seconds spent in eating at the food can in the center of the field. It is thus essentially a test of hunger behavior balanced against an exploratory tendency.

Test 9. Open field food exploration. This score is the number of marked off squares traversed during the same test period as test 8. It is similar to the open field exploration test 3, but involves the balancing of the exploratory tendency with the hunger drive.

Open field barrier tests. For this test a barrier was placed around the full food can in the center of the field. The barrier was of mesh wire up to 4½ inches and of thin sheet metal from 4½ inches up to a height of 18¾ inches. The animals were tested when hungry and were given four 3-minute subtests separated by intervals of 24 hours. Two scores were obtained under these conditions:

Test 10. Open field food barrier. This score is the time in seconds spent at the barrier around the food can. The score is thus a measure of the rat's persistence in attempting to get to food when no success is possible.

Test 11. Open field barrier exploration. This score consists of the number of marked off squares traversed during the same test period as test 10. The score is thus one of exploratory activity under conditions of a frustrated hunger drive.

4. *Emotional Defecation in the Exploratory Tests.* When placed in such situations as those just described, rats show a tendency to defecate. Hall (5) has used this defecation as a measure of emotion. In the present experiment, records were kept of the number of feces excreted by each rat in the different exploratory tests described above.

Test 4. Emotional defecation. The score is the total number of feces excreted during the last two subtests of test 1 (Dashiell maze exploration) and all subtests of test 2 (vertical maze exploration) and test 3 (open field exploration).

5. *Copulation Tests.* Twenty-four hours preceding the first 15-minute copulation subtest, all males were activated by a female in oestrum. Two copulations, or twenty minutes with the female if no copulation occurred, were allowed. This served

as a general sexual activation period to the males which had been isolated from females since the testing program had begun.

a. Technique. At the regular copulation tests, all of the males but the one to be tested were removed from the animals' living cage and a female in oestrum[2] introduced. The male was allowed one or two copulations and the female was removed. Fifteen minutes after this activation, the same female was returned to the male and a 15-minute subtest started at once. All subtests were preceded by this activation. If a male failed to copulate during the activation period preceding each 15-minute subtest he was given a second opportunity later in the evening. If he still refused to copulate, he was again offered opportunity to do so the next evening. Thus no rat was recorded as having a zero score for a 15-minute copulation subtest until he had persistently failed to copulate on two successive evenings. Four 15-minute subtests were given, with an interval of four days between subtests.

b. Tests of copulation. Three test scores were obtained from two series of copulation tests.

Test 5. Copulation test. The score for this test is the number of copulations performed during one hour of testing (four 15-minute subtests). The rats were always activated by a female in oestrum fifteen minutes before each subtest as described above.

Test 36. Copulation retest. The animals were given a second series of 15-minute subtests 127 days after the first series of test 5. Other tests had been given meantime, and, as the test schedule shows (table 1), other sex tests overlap with the subtests of the present test. The animals were activated 24 hours before the first subtest of the retest and 15 minutes before each subtest as described above. Three 15-minute subtests were given with an interval of 9 days between subtests 1 and 2, and an interval of 18 days between subtests 2 and 3. The score is the total number of copulations in the 45 minutes of testing.

Test 37. Vaginal plugs. On both of the copulation tests 5 and 36 the number of vaginal plugs delivered was recorded as well as the frequency of copulations. Since the number of plugs delivered is small, the plug scores of tests 5 and 36 have not been treated separately. The present score consists of the total number of plugs delivered in the seven 15-minute subtests of the copulation tests 5 and 36.

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[2] Prompt and willing copulation with at least three active indicator males was used as the criterion of a receptive female. Resistant, though receptive, females were not used as reward animals.

6. *Sex Barrier Apparatus.* This apparatus was a box $5\frac{3}{4}$ inches wide, $11\frac{3}{4}$ inches high, and 68 inches long, inside dimensions. Nine inches from each end a $\frac{1}{4}$ inch mesh wire barrier was placed, thus leaving a central compartment 50 inches long. The floor of this compartment was marked off into five 10-inch lengths by chalk lines. The entire apparatus was covered by hinged mesh wire doors.

a. Technique. A female in oestrum was placed in one of the wired-off end compartments and a strange male placed in the other end compartment. The male to be tested was first activated in his living cage by means of a female in oestrum or allowed 15 minutes with the female if he did not copulate. Fifteen minutes after activation in his living cage, the male was reactivated in the barrier apparatus by placing him in the end compartment with the female in oestrum. He was removed from the female end compartment after he had copulated, or after 3 minutes if he did not copulate, and immediately placed in the central compartment in the chalked-off section next to the female end compartment with his head facing away from the female. He was left in the central compartment for a test period of 4 minutes. After 2 subtests with this technique had been given it was noted that some rats tended to use the section of the central compartment in which they were placed as a home base. The technique was therefore modified for the last two subtests, and the animal to be tested was placed in the central compartment in the chalked-off section next to the strange male, facing away from the strange male.

b. Tests using the sex barrier apparatus. Two scores were obtained from the one test administered.

Test 6. Sex barrier time. This measure is essentially one of the male's interest in the female in oestrum when he is prevented from getting to her by a wire barrier. The score consists of the time the male spends in the chalked-off section next to the female end compartment facing the female. Time during which the male was in the appropriate section, but facing away from the female was not included in the score. The score is thus the total time spent near the female and facing toward her in 16 minutes of testing (four 4-minute test periods separated by 3-day intervals).

Test 7. Sex barrier distance. It appeared possible that a male might be affected by the presence of the female and yet not spend much time near her. Instead, the male may show a greater amount of activity than one not aroused by the female. This second score provides the data for determining if such an effect is present. A record was kept of the number of the chalked-off sections of the floor traversed by the male in each of the 4-minute subtests. The score for this test is thus the total number of squares traversed during the 16 minutes of testing.

7. *Jump Obstruction Apparatus.* The essential feature of this apparatus was that the animal was faced with an obstruction in the form of an empty space which he must jump to get to his reward. In its simplest form, the apparatus would consist of two elevated platforms separated by any desired distance, the animal being required to jump from one to the other for its reward. In order to prevent the influence of extraneous stimuli, the platforms of the present apparatus were enclosed. The animal was thus placed in a long, elevated box having an empty space separating the two sections of the floor. Compartment A of this box was 5 inches wide, 10 inches high and 17 inches long; the top and end were open. Compartment B was similar to compartment A but the inside width of B was as wide (5 + inches) as the outside width of A. The side boards of B were 41 inches long and thus extended about 24 inches beyond the floor board of this compartment. In use, compartment A slid between the long-side boards of compartment B, and thus a box was formed with a gap in the floor separating the two sections. The size of the gap to be jumped by the rat could be adjusted by sliding compartment A to the desired position. The entire apparatus was mounted on wooden legs (the legs set back six inches at the gap) and was thus elevated 37 inches above the floor of the room. The apparatus was covered with removable

screen wire covers of various lengths. Mesh wire was firmly fastened to the floor boards of the aparatus at each side of the gap to prevent slipping when the rats jumped or landed.

a. Technique. Handling the animals was avoided by demanding a shuttle behavior from the animals. Placed in one end of the apparatus, the animal must jump the gap to get to the reward placed at the far end of the apparatus. Having obtained this reward, he must turn and jump the gap again to obtain the reward at the other end of the apparatus. This shuttle behavior is easily established in the rat. To facilitate the administering of the reward (in the case of food) a small metal tube was inserted in each end board of the apparatus at an angle adequate to insure that dry food would roll down the tube without sticking. The tube opened into a small reward dish inside the apparatus. The reward at any given end of the apparatus was inserted while the animal was at the opposite end of the apparatus. For the thirst test, the food tubes were removed and the reward dish replaced by a flat glass plate. One drop of water was dropped to this plate for the reward at each jump.

b. Tests using the jump obstruction apparatus. Two tests were given with the jump apparatus.

Test 12. Jump hunger. The animals were given 10 days preliminary adaptation to the apparatus. For the first 5 days of this preliminary orientation, the animals were placed in the apparatus four minutes daily and allowed to eat. Food was placed at both ends of the apparatus, the amount of food in one end being such that the animal would consume it all in about two minutes and thus be forced to eat at both ends of the apparatus. The gap in the floor between the sections was 3 inches, a distance which can be stepped over by adult rats. The same procedure was followed on the sixth and seventh days, but the gap between the sections was set at 4 inches. On the eighth day of preliminary training, the animals were given 5 preliminary jumps with the shuttle technique, the gap being 4 inches. Five more preliminary jumps with the shuttle technique and a 5 inch gap were given on the ninth day. On the tenth and last day of adaptation the animals were given a preliminary four minute test with a 6½ inch gap. The jump hunger test itself was started on the next day, and consisted of four 4-minute subtests given one per day. The gap was set at 7, 7½, 8 and 8½ inches respectively for these tests, the width being increased by ½ inch daily in an effort to avoid having the obstruction effects of the gap nullified by continuous experience. The score is the total number of jumps made by the animals in the 16 minutes of testing (four 4-minute subtests).

Test 19. Jump thirst. The animals were thirsty, jumping to a water reward. For 8 days the animals were given water in the apparatus for 6 minutes daily, with a 4-inch gap between sections. On the ninth day, they were again allowed to drink for 6 minutes in the apparatus with a 5-inch gap. Eight preliminary jumps with the shuttle technique were given on the tenth day (opening 5 inches), and a 4 minute preliminary test given on the eleventh day with the opening 6½ inches wide. Starting on the twelfth day, a 4-minute subtest was given daily for 4 days. The openings were 7, 7½, 8 and 8½ inches respectively for these tests. The score is the total number of jumps made in the 16 minutes of testing.

8. *Observation Cage.* A cage 12 by 12 and 10 inches high, with 2 sides of wood, and all other sides of ½ inch mesh wire, was used to give drinking and eating tests. Three such cages were built side by side, the wooden sides being between cages. The wire floor was elevated 2 inches above the table top on which the cage rested so that spilled food could be collected.

a. Technique. The animal was placed in one of the cages for a specified time period and under the conditions to be described below.

b. Tests using the observation cage. Five measures of drinking behavior and four measures of eating behavior were obtained in the observation cages.

Drinking tests. The tip of a 50 cc. burette was bent and inserted through the wire back of the cage. The thirsty animals drank by licking the projecting glass tip, a procedure to which they were accustomed in their living cages. One four-minute preliminary drinking period was given to adapt the animals to drinking in this situation. The amount of water drunk was read directly from the burette scale. Two daily subtests of 4 minutes each were given on the two days following the preliminary period. Five days of salt water drinking (see below) were then given, followed by 2 more days of 4-minute daily drinking tests.

Three scores are based on the 16 minutes of the 4 subtests just described.

Test 14. Drinking amount. This score is the amount of water, in cc. drunk in the total 16 minutes of testing.

Test 15. Drinking amount per gram body weight. The score of each animal in the drinking test 14 was divided by that animal's body weight to obtain the present score expressed in terms of cc. per gram body weight. This score corrects for differences in body weight.

Test 16. Drinking time. The time the animal spent in drinking during each 4-minute period was recorded. The score is the time spent in drinking during the total 16 minutes of testing.

Salt water drinking tests. For these tests a 2% solution of common salt was substituted for the tap drinking water. One preliminary 4-minute period with salt water was given, then daily 4-minute test periods for four consecutive days. Two measures were obtained:

Test 17. Salt drinking amount. This score is the amount in cc. of salt water drunk in the 16 minutes of testing.

Test 18. Salt drinking time. This score consists of the time the animals spent drinking salt water during the 16 minutes of testing.

Eating tests. The animals were given a mash made by mixing 250 grams of their standard dry diet with 290 cc. of water. The mash was contained in a glass dish $3\frac{1}{4}$ by $2\frac{3}{4}$ by $1\frac{3}{4}$ inches. One 4-minute preliminary eating period was given, followed by four daily 4-minute test periods.

Test 20. Eating amount. The amount of food eaten, in grams, during the sixteen minutes of testing provides this score. A time score was not taken.

Test 21. Eating amount per gram body weight. The total amount of food eaten by each rat was divided by his body weight to give a score of grams eaten per gram body weight.

Quinine eating tests. A quinine sulphate solution was made by dissolving 5 grains of the substance in 150 cc. of water. 250 grams of dry food was mixed with 290 cc. of the quinine solution. The resulting mash contained approximately .116% of quinine.

The tests were carried out in the same manner as the other eating test. Four daily 4-minute subtests were given. Two measures were obtained.

Test 22. Quinine eating amount. The total weight, in grams, of food containing quinine eaten during the 16 minutes of testing constitutes the score for this test.

Test 23. Quinine eating time. This score is the total amount of time, in seconds, spent by the rat in eating during the 16 minutes of the quinine eating test.

9. *Sand Digging Apparatus.*[3] The essential feature of this apparatus was that the animal was required to do a specified amount of work in order to get to his reward. The type of work chosen was the digging of sand from a tube in order that the animal might pass through the tube to get to the reward compartment. The apparatus consisted of three parts:

Part 1, digging compartment. The animal was placed in a compartment $9\frac{1}{2}$ inches high, 5 inches wide and 16 inches long. The ends were of wood and extended 17 inches below the floor of the compartment so that the compartment was elevated. The floor and sides were of one-eighth inch wire mesh. The animal stood on the wire floor and dug sand from a metal tube which was inserted through a hole in the floor near one end. A sheet of tin, bent in a V-shape was mounted below the floor to collect the sand. The compartment was covered with a hinged $\frac{1}{2}$-inch mesh wire door. To give the apparatus additional solidity two wooden braces six inches wide connected the wooden ends of the apparatus.

Part 2, the sand tube. A metal drain pipe $2\frac{7}{8}$ inches in diameter and $19\frac{1}{4}$ inches long was inserted into a right elbow tube. The elbow was then cut in such a manner that it was flush with the floor of the digging compartment when inserted in the holes cut to receive it. The tube extended upward from the digging compartment at an angle of approximately 60 degrees. A strip of $\frac{1}{4}$ inch mesh wire 1 inch wide was mounted in the tube to aid the rat in climbing the incline.

Part 3, the reward compartment. This compartment was 16 by 12 by 8 inches. The floor and back were of wood, sides and top of ½ inch mesh wire. A circular opening in the front end permitted of the insertion of the exit end of the sand tube and a tin door was so arranged that it could be closed to prevent the rat from retracing down through the tube. In use, the reward compartment rested on a stand 27 inches high.

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[3] This apparatus was suggested to the writer by Professor Calvin P. Stone.

a. Technique. After the animal had been given the appropriate preliminary training, he was placed in the digging compartment. The tube was full of sand (approximately 2½ quarts of sand). As soon as the rat began to dig, a stop watch was started. The animal's task was to dig the sand from the tube. The rat could then enter the tube and run up through it to the reward compartment. The stop watch was stopped as he emerged from the sand tube.

b. Tests using the sand apparatus. Three tests were given with the sand apparatus under conditions of hunger, thirst, and sex motivation, respectively. Three scores were obtained for the hunger test.

Test 26. Sand hunger, (total time). Eleven days of preliminary training were given. For five days the animal was placed in the sand apparatus and allowed to eat for 4 minutes after he had found his way to the reward compartment through the *empty* sand tube. On the sixth day a small amount of sand was placed in the lower part of the tube. The sand did not completely fill the entrance to the tube but blocked it enough to force the rat to dig it away a little before he could enter the tube. On each of the next four days, one preliminary trial a day was given with one quart of sand in the tube; on the day following, one preliminary trial was given with the tube full of sand. On the day following this latter trial the regular test series began. No difficulty was encountered in inducing the rats to dig with the preliminary training just described. The test series consisted of 4 trials, given one per day. The score is the total time (for the 4 trials) required for the animal to dig through the sand, starting at the moment he begins to dig and ending when he emerges from the tube.

Two additional scores were obtained for the sand hunger test:

Test 24. Sand hunger digging time. This score was kept by means of an additional cumulative stop watch and consists of the time the animal spent in digging during the four test trials.

Test 25. Sand hunger idle time. By subtracting the digging time from the total time a score is obtained which represents the time the rat was in the digging compartment but not engaged in digging. During this time the rat may examine the compartment, preen, etc. The score is the sum of the idle times for the 4 trials. Tests 26, 24 and 25 are all based on the same series of 4 trials.

Test 32. Sand thirst. Since the sand thirst test occurred later in the testing program than the sand hunger test, fewer preliminary trials were given. On the first day of the preliminary adaptation for this test, the rats were given five trials to a water reward with the sand tube empty and one trial with one quart of sand in the tube. On the second day the animals were given one preliminary trial with the sand tube full and the test series started on the third day. Four test trials were given, 2 per day. The 2 trials occurring on the same day were separated by a 2½ hour period. The score is the total time required for the 4 trials.

Test 38. Sand sex. As the animals had had both the sand hunger and thirst tests before the sand sex test was given, the preliminary training for the latter test was confined to the attempt to get the animals sexually aroused in the sand apparatus. The animals were first activated in their living cages with a female in oestrum and then given 2 preliminary trials in the sand apparatus with the tube empty. One copulation (or 15 minutes with the female reward animal) was allowed after each preliminary trial. After the second preliminary trial, the male was replaced in the digging compartment with the sand tube full and given one regular test trial. All test trials were preceded by the activation and preliminary trials with the empty tube described for the first test trial. In the sand sex test, a few of the sexually inactive rats were very sluggish and their digging very desultory. In such cases the animals were removed after 30 minutes if they had not completed their trial and were given the maximum score of 30 minutes. A total of four test trials was given. Three days intervened between the first and second trials and one day between each of the remaining trials. The score was the total time required for the 4 trials.

10. *Speed Runway.* This consisted of a runway 3 inches wide, 6 inches high, and approximately 27 feet long. The top was covered with ½ inch wire mesh. For the experimenter's convenience the runway was not straight but formed a square so that the entrance and reward boxes were in the same part of the room. Sharp turns at the corners were avoided by inserting small diagonal sections 18 inches in length at each corner.

a. Technique. The animal was placed in the entrance box. As soon as he left the entrance box a stop watch was started and was stopped when the animal entered the reward box. Some retracing occurred, and if the animal returned to the entrance box, the watch was restarted when he left the box the second time. Retracings other than returns to the entrance box were not recorded because the animals were out of the experimenter's visual field in two sections of the runway. Since this test was used to obtain an estimate of the animals' maximum speed of running only the scores on the fastest 2 of the 5 daily trials were used, with the one exception noted below. While the original plan was to give 5 trials on each subtest, this proved to be impossible in the case of the sex speed test.

b. Tests using the speed runway. Three speed tests were given with hunger, thirst and sex motivation. Two scores were obtained for the hunger test.

Test 27. Speed hunger. The animals were on a reduced food diet and were tested to a food reward. Five preliminary trials (one per day) were given, the animals being allowed to eat for 6 minutes in the apparatus. The day after the fifth daily preliminary trial the rats were given 5 more preliminary trials. On the next day the regular tests were begun. Each subtest consisted of 5 trials, and one such subtest was given daily for four days. In giving the daily 5 trials, a group of five rats was given one trial, then the rats of the same group were given their second trial, etc. The trials for any one rat were thus separated by from 2 to 5 minutes. In obtaining the score for test 27 the 2 fastest trials of the daily 5 were used, and the total score is the sum of the 4 pairs of fastest trials.

Test 28. Speed hunger, slow trials. For this score, the 3 slowest trials of each daily subtest were summed.

Test 33. Speed thirst. For this test the animals were given 5 preliminary trials to the water reward. Three of the preliminary trials were given at the rate of one per day, and the last two were given on the same day. The regular series of 5 daily trials thus started on the fifth day and continued for 4 days. The score is based upon the 2 fastest trials of each of the four subtests of 5 trials each.

Test 39. Speed sex. The animals were first activated in their living cages as described under the obstruction sex test. They were then given one preliminary trial on the speed runway with a female in oestrum in the reward box. One copulation was permitted when the male entered the reward box. If the male failed to copulate, he was left with the female for one hour. The next day the first of the regular series of subtests began. The animals were again activated in their living cages and were allowed one copulation at the conclusion of the first trial. They were then given 4 more trials, copulation being prevented if possible. Forty-eight hours later the second subtest of 5 trials was given with the same activation procedure, and the third and last subtest was given 72 hours after the second subtest. While the original plan called for 5 trials on each subtest, rats that failed to copulate were so sluggish and inactive that they could not be given all 5 trials. At least 2 trials were obtained for each rat on each subtest and the majority of the rats completed their 5 trials on each test. The score is the sum of the 2 fastest trials on each of the three subtests.

11. *Columbia Obstruction Apparatus.* This was the commercial apparatus and has been fully described in the literature (13). The shocking device was also commercially made. As the shock was varied from time to time, its strength at each test is specified in table 12.

a. Technique. The Columbia investigators (13) have used a technique which allows the rat to make a maximum score of 44 crossings in a twenty-minute period. A similar technique was used in an earlier study of the writer (2) but, as pointed out there, a modified technique which would permit a greater range of scores should increase the reliability of the test. Such modifications have been introduced by Stone, Tomilin, and Barker (10). The modified technique as used in the present study consists of shortening the time required to transfer the animal from the reward compartment to the entrance compartment. This can be done providing the animals have been well adapted to the handling procedure previously to the test. Such precautions were taken in the present study, and the technique used permits of 40 crossings in a *four* minute period as compared to 44 crossings in a *twenty* minute period in the Columbia technique. A further modification was also introduced in the present study: instead of removing food or water for a specified deprivation period, the animals were placed on a reduced food or water diet so that they could be tested daily.

b. Tests using the obstruction apparatus. Six tests were given on the obstruction apparatus using the handling technique just described.

Test 13. Obstruction hunger, no shock. To familiarize the animals with the apparatus and the food reward to be obtained the animals were fed in the apparatus 4 minutes daily for 5 days. On the sixth day of the preliminary adaptation for this test the rats were given ten crossings to familiarize them with the handling procedure. Starting on the seventh day they were given one 4-minute subtest daily for four days. The score consists of the number of crossings made to the food reward in the total test time of 16 minutes. The grill was never electrified.

Test 29. Obstruction hunger, with shock. The rats were again given 5 days preliminary feeding in the apparatus (4 minutes per day, no shock). On the sixth day they were given 5 preliminary crossings without shock immediately followed by 2 preliminary crossings with shock (750 volts, 16 million ohms resistance, .047 milliamperes). If the animal failed to complete these 2 preliminary crossings with shock within 5 minutes a special procedure was used: The animal was returned to his living cage for 2 to 3 hours; he was then given 5 more preliminary crossings without shock. On the next 2 crossings the shock was turned on only after the animal had entered upon the grill. For the next two crossings the grill was electrified at all times and the animal was therefore required to enter voluntarily upon the electrified grill and cross to the food reward. If the rat still failed to complete the 2 preliminary crossings with shock within 5 minutes he was removed from the apparatus and again retested as just described two or three hours later. Nine rats failed to complete their 2 preliminary crossings with shock in 5 minutes when first tested. Seven of these rats completed the crossings as required when retested, and the two remaining rats completed their crossings on the second retest.

On the seventh day the rats were given one preliminary crossing without shock immediately followed by a preliminary 4-minute test with shock of the strength specified above. The regular 4-minute subtests began on the eighth day and continued for four days. Each subtest was immediately preceded by one preliminary crossing without shock. The strength of the shock at each subtest is specified in table 12.

Test 34. Obstruction thirst, strong shock. The rats were given 5 minutes preliminary drinking a day in the obstruction apparatus for 4 days. On the fifth day they were given 5 crossings without shock to a water reward and 2 crossings with shock (750 volts, 4 million ohms resistance, .1875 m.a.). Three rats failed to complete the 2 preliminary crossings with shock within 5 minutes, so the procedure described under the obstruction hunger test was used. All three of the animals completed their shock crossings upon the first retest. On the next three days they were given one 4-minute subtest per day. Each daily subtest was preceded by one preliminary crossing without shock. The score for this thirst test consists of the total number of crossings made in the three 4-minute subtests. The shock conditions at each subtest are specified in table 12. The shocks used for the thirst test are very strong for rats and were used because the animals appeared to be little influenced by weaker shocks. However, the shocks proved to be disruptively strong for some rats. Part of the difficulty appears to lie in the length of the grill of the commercial apparatus. In the original design (13) the grill was ten inches long but has been shortened to six inches in the commercial apparatus. With a grill this short, there appears to be little guarantee that the animal will receive a shock each time he crosses the grill. Consequently, an additional uncontrolled factor is introduced into the test.

Test 35. Obstruction thirst, with shock. Because of the apparent disruptive effects of the strong shock in the test just described, it was deemed advisable to obtain a second measure of obstruction thirst with a weaker shock. First it was necessary to readapt the animals to the apparatus. On the day following the third subtest of test 34 the rats were given 5 preliminary crossings to a water reward with no shock present. They were given no other test until 48 hours later, at which time they were given 5 preliminary crossings without shock and two with shock (800 volts, 16 million ohms resistance, .050 m.a.). All of the animals completed their 2 preliminary crossings with shock within 5 minutes on the first test. Beginning on the following day, the animals were then given three daily 4-minute subtests with shock, each daily subtest being preceded by one preliminary crossing without shock. The strength of the shock at each subtest is specified in table 12. The score is the number of crossings made in the total 12 minutes of testing.

Test 40. Obstruction sex, with shock. On the first day of the preliminary training, a female in oestrum was placed in the living cage containing 5 males. As a male copulated (only one copulation was permitted) he was removed from the living cage and placed in a small cage until 3 or 4 males had been thus activated. The 3 or 4 males were then placed, as a group, in the obstruction apparatus. The female with which they had copulated in the living cage was placed in the reward compartment of the apparatus. The males were then given two preliminary crossings without shock and were allowed to copulate with the female after each crossing. If they did not copulate they were removed after being with the female for 10 minutes. After the first group had been given their preliminary crossings a second group was activated in their living cage and the work carried out as with the first group. Males failing to copulate in their living cage were given at least three 10-minute activation periods (separated by from one to two hours) with a female in oestrum, and were then given 2 preliminary crossings in the obstruction apparatus to a female reward animal whether or not they had copulated. On the second day the animals were again activated in their living cages and given one crossing without shock, after which they were permitted to copulate with the reward animal, or given 10 minutes with the female if they did not copulate. They were then given 4 more crossings without shock (no copulation was permitted) and 2 crossings with shock (750 volts, 16 million ohms resistance, .047 m.a.). Six of the males which were inactive in copulating were very sluggish and required a much longer time to complete their preliminary crossings than the 5 minutes allowed in the hunger and thirst tests. To conserve the experimenter's time, these 6 rats were placed, as a group, in the starting compartment of the obstruction apparatus and allowed to remain in the apparatus until the preliminary crossings had been completed. Placing the animal close to and facing the grill occasionally proved successful in eliciting crossings from the inactive males, but the animal was never forced onto the grill.[4] One copulation was permitted after the second crossing with shock, and this terminated the day's work. On the next day the regular 4-minute obstruction sex subtests were begun. Before each subtest, the males were activated in their living cages by a female in oestrum as described above. They were then placed in the obstruction apparatus by groups and reactivated by a female in the reward compartment. After this activation crossing the males were removed from the apparatus for 20 minutes and were then given their 4-minute subtest with shock. Only one male was given the standard 4-minute subtest at one time. One preliminary crossing without shock immediately preceded each subtest. No copulation was allowed during the test itself, but occasionally a male would succeed in copulating before he could be removed from the reward compartment. In such cases the male was allowed to clean himself before being removed from the reward compartment and the time thus consumed was not counted in the 4-minute test. Males which would not copulate in the activation trials were tested only after they had been given several activation opportunities. Thus considerable time was spent in an effort to get all males sexually aroused at the time of the test. The behavior of the males which made more than 3 or 4 crossings on the tests indicated that the animals were definitely oriented toward the female, and few crossings of the aimless or wandering type were observed. Three 4-minute subtests were given with a 48 hour interval between subtests. The shock conditions are specified in table 12. The score is the total number of crossings made in the 12 minutes of testing.

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[4] Such facilitating procedures were, of course, never used during the standard test periods; they were employed only to conserve the experimenter's time during the preliminary adaptation periods.

Test 41. Obstruction exploration, with shock. For this test, a small Dashiell maze was attached to the reward compartment of the obstruction apparatus. As a reward for crossing the grill the animals were allowed to explore in the Dashiell maze for 3–4 seconds after each crossing. Extended preliminary adaptation was not given as this would tend to nullify the "novelty" lure of the reward. The animals were therefore given 2 preliminary crossings without shock and one with shock and the first 4-minute subtest followed immediately. Forty-eight hours later the animals were given a second 4-minute test which was immediately preceded by one preliminary crossing without shock. The shock conditions at each subtest are specified in table 12. The score for this test is the total number of crossings made in the 8 minutes of testing.

12. *Modified Warden U-Maze.* A 12-unit U-maze of the type described by Husband (6) was used. A wire door in each section prevented retracing.

a. Technique. To conserve time in the handling of animals, a turn table reward apparatus was utilized. Sixteen reward boxes were mounted on a bicycle wheel. When a rat entered a reward box the wheel was turned to a new position by means of cords extending to the experimenter's position at the entrance to the maze. It was thus possible to give one trial to each of 16 rats before it was necessary for the experimenter to readjust the reward apparatus. Time was started when the rat left the entrance runway and stopped when he entered the runway leading to the reward compartment.

b. Tests using the U-maze. Two 12-unit maze patterns were used.

U-maze A, double alternation. The units were arranged in a double alternation pattern and the true path was in the order RRLLRRLLRRLL. Preceding the testing with this maze the animals had had 26 days of testing on various hunger tests including a speed runway (see table 1). On the day following the last hunger subtest (the fourth subtest of the obstruction hunger test), the animals were given one trial on U-maze A. The rats were hungry, running to a food reward. On the next day 2 more trials were given. These three trials were considered preliminary trials and are not included in the score. On the following day and thereafter for 6 days the animals were given 5 test trials daily. On the first 2 days the daily trials were separated by a time interval of 10 to 15 minutes. The rats did not perform well with this short interval between trials, so the 5 daily trials were separated by an interval of 1½ to 2 hours on the remaining 4 days of testing. Two scores were obtained:

Test 30. U-maze A errors. This score is the total number of blind alley errors made in the 30 test trials. Retracing between units was prevented by the retracing doors, but it was possible for the animal to retrace in the same unit and thus make more than one error in that unit in a given trial.

Test 31. U-maze A time. Time was recorded from the time the animal left the starting box until he entered the runway leading to the reward apparatus. The score is the total time required for the 30 test trials.

U-maze B, simple alternation. The units were arranged in a simple alternation pattern, the true path turns being RLRLRLRLRLRL. This maze pattern was given 70 days after maze A, other tests having occurred in the interim (see schedule, table 1). The rats were hungry, running to a food reward. Two trials were given on the first day of testing with maze B. Five trials per day were given on the two following days. By this time, practically all the rats were making perfect scores so the test was discontinued. Trial number one was considered a preliminary trial, the remaining 11 trials were considered test trials. Two measures were obtained:

Test 44. U-maze B errors. The score is the total number of errors made on the 11 test trials.

Test 45. U-maze B time. The score is the total time for the 11 test trials, recorded as for the maze pattern A.

13. *Multiple Light Discrimination Apparatus.* A Stone (8) 5 unit multiple light discrimination apparatus was used.

a. Technique. Food was removed 48 hours before the first trial on the light discrimination test. Thereafter, motivation was controlled by permitting the animals to eat for a limited time period daily. The length of the eating period was varied in accordance with body weight records. The animals were given 7 days on the discrimination apparatus, the lighted side being the correct choice. The number of trials per day was 1,2,1,2,4,5,5. This distribution of trials was dictated by the experimenter's ability to give the entire group of animals the number of trials indicated. While the distribution of trials is not uniform on successive days, it was entirely uniform for all rats. The position of the light for each unit for each trial was determined according to the chance series prepared by Stone. The turn-table reward apparatus described for the U-maze was also used with the discrimination apparatus.

b. Tests using the light discrimination apparatus. Two scores were obtained on the one test given with the light discrimination apparatus.

Test 42. Light discrimination errors. The score is the total number of errors made on the 20 trials given.

Test 43. Light discrimination time. The total time required for the 20 trials given constitutes this score.

14. *Activity Cages.* Slonaker (9) type activity cages were used. A sleeping cage containing food and water was suspended from the axle of the drums.

a. Technique. The animals were 361 days old at the time of the activity drum tests. Since Shirley (7) found that older rats require longer to strike their stride in activity drums than younger rats, the rats of the present experiment were kept in the drums for a total period of 8 weeks. In order to equate for possible differences in the different drums, each rat was placed in a different drum weekly.

Test 47. Activity. The score consists of the total number of revolutions made in the last 4 weeks the animals were in the drums. During this period many of the animals became ill and some died. When the animals were removed from the drums they were rated as to general health and appearance,[5] and only rats which were in good health at that time have been considered in this study. The number of animals upon which the activity correlations are based has thus been reduced to 34.

15. *Body Weight Scores.* In order to get a weight representing the animals' condition throughout the testing schedule, body weights at four different periods of the testing program were summed and provide the score for body weight correlations.

Test 46. Body weight. The weights used were taken at periods during which the rats were on neither food nor water deprivation. The four weights were taken at the ages of 179, 204, 224 and 267 days.

III. RESULTS

The results obtained will be discussed under four headings, namely: A. Means and Measures of Variability; B. Distributions and Special Scoring; C. Reliability Coefficients; and D. Intercorrelations.

A. Means and Measures of Variability

The means for each subtest and the means and measures of variability for the total score of each test are presented in tables 2 to 16. For convenience in presentation, the tests have been grouped according to the type of apparatus or drive involved.

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[5] Professor Calvin P. Stone very kindly made these health ratings. The writer also wishes to express his gratitude to James Stone who took care of the animals while they were in the drums and took all records during this period.

The tests thus grouped will be considered in an approximately chronological order. Standard deviations are presented for the tests for which they could be conveniently determined. In tests which necessitated special scoring (see p. 47), standard deviations are not presented.

The original group of animals consisted of 55 male rats but this number was reduced by deaths during the course of experimentation. Four rats died early in the testing program and their data have not been included in the data presented. Two rats which died late in the testing program have been included in the results of the tests which they completed. Several more deaths and numerous illnesses further reduced the size of the group during the last test given, namely, the activity drum test. The number of cases is thus 51 for most of the tests. Deviations from this number will be indicated at the tests involved. Inasmuch as the data presented in this section represent by-products of the study of intercorrelations, they will be considered only briefly rather than in detail.

1. *Tests of Exploratory Activity.* The means and measures of variability for tests involving mainly exploratory activity are presented in table 2. The means of the subtests of the Dashiell maze, test 1, and the vertical maze, test 2, fluctuate but not in a consistent direction. The open field scores, test 3, tend to decrease with repetition. A decrease with successive tests is probably to be expected in exploratory tests, and the simpler the situation (other factors being constant) the more rapidly might this decrease in scores be expected. The Dashiell and vertical maze tests offer a more complicated situation to be explored than does the open field, and hence a more or less constant score is obtained in tests 1 and 2.

The open field food exploration test 9 also shows a decrease in score with successive subtests. This decrease is to be expected, not only on the basis of the satiation of the exploratory drive, but also on the basis of conflict with the hunger drive. The animals were hungry in an open field with food available in the center. The rats may be expected to spend more time in eating with repetition of the test and hence would have less time for exploring.

With the open field barrier exploration test 11, the situation becomes reversed and successive tests show an increase in exploratory activity. The animals were hungry but were prevented from obtaining food by a wire barrier about the food can. The animals might then spend their time in a vain effort to obtain the food or in running about the open field. As the results show, the rats tend to explore more with successive retests.

2. *Emotional Defecation.* The scores for test 4 were obtained from the Dashiell maze test 1, the vertical maze test 2, and the open field test 3. The score consists of the number of feces excreted by the rats during the subtests of the tests just mentioned. No defecation scores were obtained for the first two subtests of the Dashiell maze test 1, and the defecation measure is therefore based on scores obtained on ten subtest periods. The mean defecation scores for these subtests and other relevant data are presented in table 3. In general, there is a decrease in defecation and an increase in the number of rats not defecating with successive subtests on the same apparatus. Placing the animals in a new situation again results in more defecation, with a later decrease if this new situation is repeated. Hence the emotional adaptation indicated by the decreasing scores in a given situation appears to be more or less specific to that situation, and does not appear to be an emotional adaptation to new situations in general.

TABLE 2

Mean scores and measures of variability for tests of exploratory activity ($N = 51$)

Scores are in terms of the number of units (marked off squares or pegs) explored during the test periods.

TEST NUMBER	NAME OF TEST	SUBTEST LENGTH	MEANS OF SUBTESTS				TOTAL TEST SCORE		
			1	2	3	4	Mean	Range	$\sigma_{\text{dis.}}$
		minutes							
1	Dashiell maze	4	99	107	105	100	410	126–581	90.3
2	Vertical maze	4	32	29	36	34	132	6–259	61.4
3	Open field	3	94	91	76	84	345	114–570	96.4
7	Sex barrier distance	4	40	35	35	35	145	76–209	25.7
9	Open field food	3	73	70	66	55	264	100–440	79.6
11	Open field barrier	3	89	107	108	111	415	298–595	66.7

However, the data of the present study are not adequate to establish the point, since the animals may have defecated more on some situations if they had not had previous experience in other situations. The rats appear to be more emotionally aroused in the open field test than in the other situations used. It is to be noted that a large number of rats fail to defecate at any given subtest, and that 12 of the 51 rats failed to defecate at any time during the series of tests.

3. *Copulation Tests.* The means and measures of variability for the copulation tests and the vaginal plug score are presented in table 4. The results for the copulation test 5 have been considered in detail in an earlier paper (1). The copulation retest 36 scores for frequency of copulations are consistently lower than those of the original test 5. The long period of sexual deprivation preceding the retest, the intervening experience including food and water deprivations, and the greater age of the rats on the time of the retest, may all be factors tending to decrease the frequency of copulations at the time of the retest. The lower average frequency of copulations at the retest is not due to an increase in the number of non-copulating rats. As the table shows, the number of non-copulating rats on any subtest of the

TABLE 3

Mean scores and ranges for the defecation test 4, obtained from exploratory tests 1, 2, and 3 (N = 51)

SCORE IN TERMS OF:	DASHIELL MAZE, TEST 1		VERTICAL MAZE, TEST 2				OPEN FIELD, TEST 3				TOTAL SCORE TEST 4
	Subtests										
	3	4	1	2	3	4	1	2	3	4	
Mean number of feces..........	.73	.49	1.57	1.25	.88	.73	2.80	1.82	2.08	1.82	14.2
Number of rats not defecating.	42	44	34	37	41	42	15	31	27	32	12
Mean number of feces, defecating rats.......	4.1	3.6	4.7	4.6	4.5	4.1	4.0	4.7	4.4	4.9	18.5
Range of number of feces........	0–7	0–6	0–9	0–7	0–6	0–8	0–8	0–8	0–8	0–9	0–50

retest is lower than the number of non-copulating rats of the corresponding subtests of the original copulation test 5. While 3 rats failed to copulate on any subtest in test 5, no rat failed to copulate on at least one subtest in the retest 36.

The frequency of vaginal plug deliveries is less on the first retest subtest than on the subtests of the original test, but in the remaining subtests of test 36 the vaginal plug frequency is equal to that of the original tests. Hence, a decrease in copulation frequency is not necessarily accompanied by a decrease in vaginal plug frequency, although, as will be shown later, the two measures have a fairly high intercorrelation. The reduced vaginal plug frequency at the first subtest of the retest is probably to be attributed to the long sexual deprivation period preceding the retest. There were more rats failing to deliver plugs on this first retest than on any other subtest. In general, more rats failed to deliver plugs on the retest than on the original test, but those which did deliver plugs tended to deliver more. In the total 7 subtests involved in tests 5 and 36, only two cases occurred in which 3 plugs were delivered in any one 15-minute subtest, and both of these cases occurred on the third subtest of the copula-

TABLE 4

Mean scores and measures of variability for the copulation test scores (N = 51)

TEST NUMBER	NAME OF TEST	TEST IN TERMS OF:	MEANS OF SUBTESTS*				TOTAL TEST SCORE		
			1	2	3	4	Mean	Range	σdis.
5	Copulation	Number of copulations	10.3	10.6	12.6	11.3	44.8	0–98	24.9
	Vaginal plugs of test 5	Number of plugs	.73	.75	.88	.90	3.26	0–7	
36	Copulation retest	Number of copulations	8.7	8.8	9.6		27.1	5–50	11.6
	Vaginal plugs of test 36	Number of plugs	.51	.80	88		2.20	0–6	
37	Vaginal plugs, tests 5 and 36	Number of plugs					5.45	0–12	3.62
Number of rats failing to copulate on test 5			8	7	3	3			
Number of rats failing to copulate on test 36			5	4	1				
Number of rats failing to deliver plugs on test 5			19	19	15	14			
Number of rats failing to deliver plugs on test 36			28	17	22				

* The data presented in an earlier paper (1) were based on 55 cases and the slight differences between the means reported in that study and the present one are due to this difference in the size of the two groups.

tion retest 36. In the original test 5, eleven rats failed to deliver a plug on any subtest, while fifteen rats failed to deliver a plug on any subtest of the retest 36. While no rat failed to copulate on at least one of the 7 subtests of the two copulation tests 5 and 36, eight rats failed to deliver a plug at any time during the testing.

4. *Sex Barrier Time.* The scores for the time in seconds spent at the female end of the apparatus during each 4-minute subtest of the sex barrier time test 6 are presented in table 5. As the table shows, there is an increase in time spent near the female on the second subtest. This increase may be due to a tendency of the rats to use the section of the apparatus in which they are placed as a "home." On subtests 1 and 2, the male to be tested was placed in the apparatus near the female end compartment, while on subtests 3 and 4 the male was placed in the apparatus near the strange male compartment. This change in technique is probably adequate to account for the drop in score at subtest 3. Subtest 4 shows an increase of score over subtest 3. This increase may be due to a readaptation to the general test situation after the change in technique at subtest 3.

TABLE 5

Mean scores and measures of variability for the sex barrier time test, test 6 ($N = 51$)

Scores are in terms of the number of seconds spent near and facing the female in each subtest of 4 minutes.

TEST NUMBER	SCORE IN TERMS OF	MEANS OF SUBTESTS				TOTAL TEST SCORE		
		1	2	3	4	Mean	Range	$\sigma_{dis.}$
6	Seconds near female	104	119	92	101	416	228–647	104.6

5. *Jump Tests.* Mean scores for the two tests using the jump obstruction apparatus are presented in table 6. The scores for both the hunger and the thirst tests tend to increase somewhat with successive retests. Many more jumps were made on the thirst test 19 than on the hunger test 12, but the jump thirst test was given later in the program than the jump hunger test and the higher thirst scores may be due to the previous experience on the apparatus at test 12. The average body weight at

the first jump hunger subtest was 233 grams, at the first jump thirst subtest it was 256 grams. The increased thirst scores cannot therefore be attributed to an increase in motivation due to greater loss in body weight.

6. *Drinking Tests.* The means by subtests and total scores for tests of drinking behavior are presented in table 7. The amount drunk and the time spent drinking, tests 14 and 16, tend to increase with successive subtests. The scores for salt drinking amount and salt drinking time, tests 17 and 18, remain relatively constant at the different subtests. The absolute amount drunk and the time spent drinking is less, however, for the salt drinking tests than for the tap water drinking tests.

TABLE 6

Mean scores and measures of variability for tests using the jump obstruction apparatus (N = 51)

The score is in terms of the number of jumps made. Each subtest is 4 minutes in length.

TEST NUMBER	TEST NAME	MEANS OF SUBTESTS				TOTAL TEST SCORE		
		1	2	3	4	Mean	Range	$\sigma_{dis.}$
12	Jump hunger	7.0	7.2	8.4	8.7	31.2	2–71	13.8
19	Jump thirst	17.8	21.3	24.4	23.9	87.4	0–144	31.5

TABLE 7

Mean scores and measures of variability for tests of drinking behavior (N = 51)

All subtests are 4 minutes in length

TEST NUMBER	NAME OF TEST	SCORES IN TERMS OF:	MEANS OF SUBTESTS				TOTAL TEST SCORE		
			1	2	3	4	Mean	Range	$\sigma_{dis.}$
14	Drinking amount	Cc. drunk	6.2	7.2	7.3	7.5	28.3	19–40	6.2
15	Drinking per gm. wt.	Cc. per gm. wt.					.110	.08–.15	.015
16	Drinking time	Seconds drinking	148	168	184	188	688	557–867	113
17	Salt drinking amt.	Cc. drunk	4.4	4.3	4.7	4.4	17.8	8–26	4.1
18	Salt drinking time	Seconds drinking	108	102	108	105	423	205–612	91

7. *Eating Tests.* The means by subtests and total scores are presented in table 8. With the exception of the open field food barrier, test 10, all of the tests yield higher scores with successive subtests. Test 10 differs from the other eating tests in that it

involves no actual eating, the score is the time in seconds that the animal will spend near food when he is prevented from getting to the food by a wire barrier. The decrease in score for test 10 with successive subtests indicates that, if frustrated in his efforts to get food, the rat will spend less and less time in an attempt to get at the barred food. The rats eat relatively less food containing quinine, test 22, but the sharp rise in amount of quinined food eaten in subtests 3 and 4 indicates that the effects of the quinine of the concentration used were rapidly disappearing.

TABLE 8

Mean scores and measures of variability for tests of eating behavior ($N = 51$)

Each subtest of tests 8 and 10 is 3 minutes in length; each subtest of tests 20, 21, 22 and 23 is 4 minutes in length.

TEST NUMBER	NAME OF TEST	SCORE IN TERMS OF:	MEANS OF SUBTESTS				TOTAL TEST SCORE		
			1	2	3	4	Mean	Range	$\sigma_{dis.}$
8	Open field food eating	Seconds eating	66	84	98	108	356	141-557	109.6
10	Open field food barrier	Seconds at barrier	92	51	44	30	217	101-342	51.2
20	Eating amount	Grams eaten	5.3	5.9	6.9	7.2	25.3	4-44	9.7
21	Eating amt. per gm. wt.	Gms. per gm. wt.					.093	.02-.16	.032
22	Quinine eating amount	Grams eaten	2.2	3.7	5.7	6.8	18.4	0-41	11.6
23	Quinine eating time	Seconds eating	140	159	192	207	698	211-892	

TABLE 9

Mean scores and ranges for all tests using the sand digging apparatus ($N = 51$)

All scores are in terms of seconds. One trial constitutes each subtest.

TEST NUMBER	NAME OF TEST	MEANS OF SUBTESTS				TOTAL TEST SCORE	
		1	2	3	4	Mean	Range
24	Sand hunger digging time	190	164	171	173	698	528–1277
25	Sand hunger idle time	75	61	61	54	252	91–1031
26	Sand hunger total time	265	226	232	228	950	675–2308
32	Sand thirst total time	282	262	281	254	1079	594–2400
38	Sand sex total time	385	407	386		1179	491–5400

8. *Sand Tests.* The means and measures of variability for all tests using the sand apparatus are presented in table 9. As the table shows, the animals required more time to dig through the sand on the first subtests of the sand hunger measures than on the later subtests. The decrease in score from the first to last

subtest is greater for hunger idle time than for hunger digging time. More time was required on the first and third subtests of the sand thirst test 32 than on the second and fourth subtests. This difference may be due to the distribution of trials for this test; the first two subtests were given on one day and the last two subtests on the following day. Less time was required by the hungry animals to dig through the sand to a food reward than for the same animals to dig, when thirsty, to a water reward, even though the factor of previous experience should favor the thirst test. The average body weight at the time of the sand hunger test was 253 grams, at the thirst test it was 259 grams.

TABLE 10

Mean scores and ranges for all tests using the speed runway (N = 51)

Five trials on the runway constitute each subtest. Scores are in terms of average seconds per trial for the trials indicated.

TEST NUMBER	NAME OF TEST	SCORE IN TERMS OF:	MEANS OF SUBTESTS				TOTAL AVERAGE SCORE PER TRIAL	
			1	2	3	4	Mean	Range
28	Speed hunger slow trials	3 slowest trials	17.0	14.9	14.2	11.9	14.5	9–30
27	Speed hunger	2 fastest trials	10.2	9.6	9.3	8.3	9.4	7–15
33	Speed thirst	2 fastest trials	9.0	8.5	8.2	7.6	8.3	6–12
39	Speed sex	2 fastest trials	12.8	11.6	10.2		11.5	7–32

The rats required, on the average, much more time to dig through the sand to a sex reward than to either food or water rewards. Some rats, however, dug as rapidly to get to a female in oestrum as they did to get to food or water, but rats which were sexually sluggish took a much longer time and a few animals even failed to finish their trial in the arbitrary time limit of 30 minutes. No such failures occurred in either the hunger or thirst tests.

9. *Speed Tests.* Table 10 presents the mean score data for tests using the speed runway. In all of the tests, there is a gradual increase in speed (decrease in time score) with the successive subtests, indicating that the rats had probably not reached their maximum speed in any of the tests. The thirsty rats ran faster to a water reward than they did to a food reward

when hungry. But as the thirst test 29 was given later in the testing program than the hunger test 27, the lower time scores of the thirst test may be due to the factor of greater experience on the apparatus. The speed sex test 39 was given still later in the program than the thirst test, but in spite of the advantage of the greater experience with the apparatus, the time scores for the sex test are higher than for either the hunger or thirst scores.

10. *Obstruction Tests.* Mean scores and measures of variability for all tests using the Columbia obstruction apparatus are presented in table 11. The strength of shock was varied for successive subtests in an effort to prevent too rapid adaptation to the shock and consequent loss of the inhibiting effect of the shock. The shock conditions at each subtest are specified in table 12. With the exception of the sex and exploratory tests, the shocks used in the present study are much stronger than the standard shock used by the Columbia investigators (13). With a somewhat stronger shock (475 volts, 8,750,000 ohms resistance, .054 m.a.) Warden and Nissen found that their animals refused to cross the grid. As table 11 shows, the rats of the present study crossed the grill 9 or 10 times in a four minute period (obstruction thirst, strong shock, test 34) with a shock of .200 milliampere

TABLE 11

Mean scores and measures of variability for all tests using the Columbia obstruction apparatus (N = 51)

Scores by subtests are in terms of the number of crossings made in the 4 minute period. Total scores are based upon the total number of crossings made in all subtests.

TEST NUMBER	NAME OF TEST	MEANS OF SUBTESTS				TOTAL TEST SCORE		
		1	2	3	4	Mean	Range	σ dis.
13	Obstruction hunger, no shock	21.0	23.1	25.0	27.2	96.3	57–133	15.2
29	Obstruction hunger, with shock	20.5	20.5	17.4	19.9	78.2	26–120	24.5
34	Obstruction thirst, strong shock	9.9	9.4	10.1		29.4	1–58	
35	Obstruction thirst, with shock	16.2	16.6	17.8		50.6	14–103	18.4
40	Obstruction sex, with shock	10.9	12.9	12.5		36.3	1–82	20.7
41	Obstruction exploratory, with shock	3.9	3.6			7.5	0–30	7.1

(table 12). It must be understood that the number of crossings made in the present study cannot be compared to the number of crossings obtained in the Columbia studies because of the marked differences in handling technique (see above p. 26). But the fact that the animals of the present study consistently crossed a grill bearing a shock several times as strong as the shock which completely inhibited crossing the grill in Warden and Nissen's study requires some explanation. Age and stock differences do not appear to offer an adequate explanation, but the difference in the length of the grill in the apparatus originally used by the Columbia investigators (grill 10 inches long) as compared to the commercial apparatus (grill 6 inches long) may play an important rôle. As previously indicated, some of the rats do not appear to receive a shock each time they cross the short grill. A second factor which is probably of considerable importance in accounting for the difference under consideration is that of preliminary adaptation to the apparatus and the reward it contains. In the Columbia studies the animal's only experience with the apparatus before the test consisted of the 5 preliminary crossings given

TABLE 12

Specification of shock conditions at each subtest for all tests using the Columbia obstruction apparatus

A.C. current of 60 cycles

TEST NUMBER	NAME OF TEST	SUBTESTS	VOLTS	MILLIONS OF OHMS RESISTANCE	MILLIAMPERES
13	Obstruction hunger, no shock	1, 2, 3, 4	000	00	.000
29	Obstruction hunger, with shock	1	800	10	.080
		2, 3, 4	800	8	.100
34	Obstruction thirst, strong shock	1, 2	800	4	.200
		3	800	8	.100
35	Obstruction thirst, with shock	1	800	13	.062
		2, 3	800	10	.080
40	Obstruction sex, with shock	1, 2, 3	750	16	.047
41	Obstruction exploratory, with shock	1	750	16	.047
		2	600	16	.038
	Studies by the Columbia investigators (14)		475	10.1	.047

immediately preceding the test. In the present study, extensive preliminary adaptation to the apparatus and reward involved was given, and it appears probable that rats thoroughly familiar with the apparatus will stand a shock which would completely inhibit all crossings if they were unfamiliar with the apparatus. Stone, Tomilin and Barker (10) have used a shock (800 volts, 13 million ohms resistance, .0615 m.a.) much stronger than the completely inhibiting shock of Warden and Nissen and were nevertheless able to obtain from 28 to 36 crossings in a ten minute period to a sex reward. Their technique also involved a more adequate preliminary adaptation to the apparatus than the Columbia technique provides.

The influence of adaptation to the apparatus and to the handling techniques is indicated in the rise of successive subtest scores of the obstruction no shock hunger test 13. The obstruction tests given with shock do not show any marked tendency to yield higher mean scores with successive subtests; the techniques used thus proved to be adequate to hold the mean scores of subtests at an approximately constant level. More crossings were obtained with hunger motivation, test 29, than with thirst, test 35, although the shock was weaker at the thirst test. However, the use of the strong shock in the thirst test 34 may have resulted in negative conditioning which carried over to the thirst test 35 with weaker shock. The fact that the number of crossings increases slightly with successive subtests in the thirst test 35 might indicate a weakening of any inhibition due to negative conditioning. Fewer crossings were made to a sex reward, test 40, than to water, test 35, or food, test 29. The smallest number of crossings was made in the obstruction exploratory test 41.

11. *Learning Measures.* The mean scores and ranges for all learning tests are presented in table 13. As indicated in the table, the U-maze pattern B (simple alternation) proved to be very easy for the animals, and the restricted range of scores may be expected to result in lower reliability.

12. *Activity Drum Measures.* The means and ranges for each week in the activity drums are presented in table 14. Since the animals were middle aged (361 days) at the time they were

placed in the drums, a relatively low amount of activity was to be expected. As the table indicates, rats of the age used require a long time in the drums before much stability in mean scores is obtained. In the present case the mean activity increased for 5 weeks. The score, test 47, used for correlations consists of the activity during the last 4 weeks of the 8 weeks in the drums. As previously stated, many deaths and illnesses occurred before the completion of the activity drum test. A total of 12 rats of the original group of 55 rats (including those which died before beginning the activity test) died before the completion of the test, and 9 rats were considered to be in such poor health at the end of the activity test that their scores have been omitted. The results for the activity measure are therefore based upon 34 animals.

TABLE 13

Mean scores and ranges for learning measures

TEST NUMBER	NAME OF TEST	N	SCORE BASED ON TRIALS	TOTAL TEST SCORE	
				Mean	Range
30	U-maze A, errors	51	4–33	111.4	45–173
31	U-maze A, time in seconds	51	4–33	1714.2	842–3929
44	U-maze B, errors	49	2–12	15.9	5–38
45	U-maze B, time in seconds	49	2–12	378.1	225–858
42	Light discrimination, errors	50	1–20	33.7	20–62
43	Light discrimination, time	50	1–20	1055.9	555–2140

TABLE 14

Mean scores and ranges for the activity drum measure ($N = 34$)

TEST	WEEK IN DRUM	MEAN REVOLUTIONS PER WEEK	RANGE
Preliminary	1	2280	30– 5,047
Preliminary	2	2093	1– 7,026
Preliminary	3	2722	0–10,418
Preliminary	4	3383	2–13,715
Test 47, subtest 1	5	3876	2–16,794
Test 47, subtest 2	6	5342	0–26,422
Test 47, subtest 3	7	4508	0–29,089
Test 47, subtest 4	8	4986	0–27,383
Test 47, total score, average	5-8	4678	12–24,054

13. *Body Weight Measures.* The body weight measures, test 46, are presented in table 15. Each of the four weights used was taken after the animals had been on an unrestricted food and water diet for several days. Deprivation periods occurred at the ages indicated in the test schedule, table 1, and the weights presented do not therefore represent growth weights of animals on full diet. Since the animals were six months old before they were placed on any restricted food or water diets, it may be assumed that they had passed their period of rapid growth. As the table indicates, the body weights at the ages stated remained about constant. Failure of the animals to show any increase in weight may be attributed to the intervening periods of restricted diet.

TABLE 15

Mean scores and ranges for body weight measures (N = 51)

BODY WEIGHT	MEAN WEIGHT	RANGE
	grams	
Weight at 179 days of age	301	220–370
Weight at 204 days of age	285	200–348
Weight at 224 days of age	296	192–360
Weight at 267 days of age	298	188–365
Test 46, body weight (average of above 4 weights)	295	200–358

14. *Comparison of Strength of Drives.* Although the data of the present study were not taken for the purpose of ranking the different drives according to strength and are not entirely suitable for such a comparison, some indication as to relative rank may be obtained. In several cases, the same apparatus was used to measure two or more different drives and thus some comparison is possible. It should be remembered, however, that two drives thus measured are never equivalent from the standpoint of previous experience. Furthermore, the techniques were not always identical. The results can therefore be considered only as suggestive or confirmatory of other work, and for this reason the significance of the differences between the various tests has not been calculated.

In all cases in which more than one drive was tested on the same apparatus, the order of testing was uniformly hunger, thirst, sex, exploration. These tests were not given consecutively but were usually separated by an interval of several weeks. The data given in table 16 have already been presented in tables 6, 9, 10 and 11, but are reorganized in table 16 to facilitate comparisons of the strength of the drives. When the number of subtests varied from drive to drive, the score has been changed to a uniform basis. As the table shows, exploration is the weakest drive. Three different measures show the sex drive to be weaker than either hunger or thirst, in spite of the fact that the sex tests are favored by position in the testing program. Two measures show thirst to be stronger than hunger, and two measures show the reverse relation. Various factors which probably influenced the scores made on the hunger and thirst tests have been previously discussed. The data of the present study thus tend to confirm the results obtained by Columbia investigators with the obstruction apparatus; i.e. thirst and hunger appear to be approximately equal in strength, sex is weaker and exploration weakest of all.

TABLE 16

Mean scores arranged to show relative strength of drives

Based on data of tables 6, 9, 10 and 11

TYPE OF TEST	SCORE IN TERMS OF:	MOTIVATION			
		Hunger	Thirst	Sex	Exploration
Jump obstruction	Mean jumps in 16 minutes	31.2	87.4		
Sand tests	Mean seconds per trial	238	270	393	
Speed tests	Mean seconds per trial	9.4	8.3	11.5	
Columbia obstruction	Mean crossings in 4 minutes	19.6	16.9	12.1	3.8

B. Distributions and Special Scoring

In an earlier study (2) the writer found that a number of tests yielded raw scores which were so atypical in distribution as to make a logarithmic scoring desirable before using the data for correlational purposes. In the present study many distributions were so markedly skewed as to render a rescoring desirable. The marked skewness is especially characteristic of tests involving time scores for a given task rather than a uniform time period. Logarithmic scoring proved to be inadequate to render such skewed distributions approximately normal, and a method involving reciprocals was used.[6] This method results in distribution curves which are approximately normal and reduces the effect of the few extreme rats which deviate markedly from the group. The following tests have been subjected to the modified scoring: Quinine eating time, test 23; all tests using the sand apparatus, tests 24, 25, 26, 32 and 38; all tests using the speed runway, tests 27, 28, 33 and 39; light discrimination measures, tests 42 and 43; U-maze A time, test 31; U-maze B measures, tests 44 and 45; emotional defecation, test 4; body weight, test 46; and activity, test 47. Two tests have a somewhat bimodal distribution due to an excess number of zero scores which, of course, no method of rescoring can eliminate. In the vaginal plug test 37, eight rats failed to deliver a plug at any time, and in the emotional defecation test 4, twelve rats did not defecate on any of the tests.

C. *Reliability Coefficients*

Thirty of the measures obtained are each based upon 4 subtests, with an interval of at least one day between any two successive subtests. For all tests based upon 4 such subtests, the reliability coefficients have been obtained by correlating the

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[6] This method involves equating the range of scores of the 10 or 12 rats making the highest scores with the range of the 10 or 12 rats making the lowest scores. The equation, Y (modified score) $= \frac{1}{a - x}$, where x is the raw score, was used. By equating the ranges of scores just described, the following equation is obtained: $Y_{h_1} - Y_{h_2} = Y_{L_1} - Y_{L_2}$, where Y_{h_1} is the highest score made by any rat, Y_{h_2} is the lowest score of the rats falling in the high range group, Y_{L_1} is the highest score of the rats of the low range group, and Y_{L_2} the lowest score made by any rat. Substituting the value of Y from the first equation:

$$\frac{1}{a - x_{h_1}} - \frac{1}{a - x_{h_2}} = \frac{1}{a - x_{L_1}} - \frac{1}{a - x_{L_2}}$$

from which the value of the constant a may be determined. The class interval points of the modified scores are then determined and converted into raw score terms.

sum of the scores on the first and third subtests with the sum of the scores on the second and fourth subtests. Reliability coefficients thus obtained are presented in table 17. Twenty-one of the thirty reliability coefficients (corrected) of this table lie between .85 and .99, and four more are between .80 and .85. The

TABLE 17

Reliability coefficients for tests having four subtests

Reliability coefficients obtained by correlating scores on the first and third subtests with scores on the second and fourth subtests.

TEST NUMBER	TEST NAME	SCORES IN TERMS OF:	N	r OF SUBTESTS 1 + 3 VS. 2 + 4	SPEARMAN-BROWN r
1	Dashiell maze exploration	Squares traversed	55	.798	.888
2	Vertical maze exploration	Pegs traversed	55	.869	.930
3	Open field exploration	Squares traversed	55	.770	.870
5	Copulation	Copulations	55	.870	.931
	Plugs of test 5	Vaginal plugs	55	.789	.882
6	Sex barrier time	Seconds near female	55	.502	.669
7	Sex barrier distance	Squares traversed	55	.612	.759
8	Open field food eating	Seconds eating	55	.723	.839
9	Open field food exploration	Squares traversed	55	.758	.863
10	Open field food barrier	Time at barrier	55	.631	.774
11	Open field barrier exploration	Squares traversed	55	.835	.910
12	Jump hunger	Number of jumps	55	.879	.935
13	Obstruction hunger, no shock	Number of crossings	55	.768	.869
14	Drinking amount	Cc. drunk	55	.820	.901
16	Drinking time	Seconds drinking	55	.704	.826
17	Salt drinking amount	Cc. drunk	55	.741	.851
18	Salt drinking time	Seconds drinking	55	.693	.819
19	Jump thirst	Number of jumps	55	.952	.975
20	Eating amount	Grams eaten	55	.854	.921
22	Quinine eating amount	Grams eaten	55	.850	.919
23	Quinine eating time	Seconds eating	55	.812	.895
24	Sand hunger digging time	Seconds digging	55	.857	.923
25	Sand hunger idle time	Seconds idling	55	.614	.761
26	Sand hunger total time	Seconds through sand	55	.854	.921
27	Speed hunger fast trials	Two fastest trials	55	.667	.800
28	Speed hunger slow trials	Two slowest trials	55	.505	.671
29	Obstruction hunger with shock	Number of crossings	55	.835	.910
33	Speed thirst	Two fastest trials	52	.894	.944
46	Body weight	Grams	51	.976	.988
47	Activity drums	Revolutions	34	.943	.971

reliability coefficients of the sex barrier time test 6 and of the speed hunger slow trials test 28 are the lowest, r = .67; the sex barrier distance test 7, the open field food barrier test 10, and the sand hunger idle time test 25 lie in the range .75 to .80.

Eight measures were based upon 2 or 3 subtests with an interval of at least one day between any two successive subtests. To obtain the reliability coefficients for these tests, the intercorrelations between subtests were calculated. The mean of the intercorrelations of the given test was then determined and corrected by the Spearman-Brown formula to determine the reliability of a test of the length used. Table 18 presents the reliability coefficients thus obtained. The lowest reliability in this table is that of .78 for the vaginal plug score of test 36 and is probably to be attributed to the very limited range of the vaginal plug scores. The reliability of the obstruction thirst strong shock, test 34, is considerably less (.84 compared to .97) than the reliability of the obstruction thirst test with a more moderate shock, test 35. The difference in the reliability of these two tests can probably be best accounted for in terms of the disruptive effects of the strong shock.

TABLE 18

Reliability coefficients for tests having less than four subtests

TEST NUMBER	NAME OF TEST	SCORE IN TERMS OF:	N	r SUBTEST 1 VS. 2	r SUBTEST 1 VS. 3	r SUBTEST 2 VS. 3	MEAN r	SPEARMAN-BROWN r
34	Obstruction thirst strong shock	Number of crossings	51	.735	.554	.611	.633	.838
35	Obstruction thirst	Number of crossings	52	.901	.897	.904	.901	.965
36	Copulation	Number of copulations	52	.772	.649	.620	.680	.865
	Vaginal plugs of test 36	Number of plugs	52	.586	.429	.624	.546	.783
38	Sand sex	Seconds through sand	52	.807	.813	.822	.814	.929
39	Speed sex	2 fastest trials	52	.808	.742	.855	.802	.924
40	Obstruction sex	Number of crossings	52	.665	.709	.766	.713	.882
41	Obstruction exploration	Number of crossings	51	.757				.862

Reliability coefficients for the learning measures were obtained in the usual way by correlating the scores on odd trials with the scores on even trials and are presented in table 19. The reliabilities for the U-maze B measures are relatively low for maze performance, and are probably due to the ease with which the animals mastered this pattern and the resulting reduced range of scores. Increasing the number of trials would probably not have increased the reliability of this pattern to any great extent since but 9 of 50 rats made errors on the last trial given. The reliability coefficients of the other learning measures fall within the general range of coefficients reported in the literature.

TABLE 19

Reliability coefficients for learning measures obtained by correlating scores on odd trials with scores on even trials

TEST NUMBER	NAME OF TEST	N	SCORE BASED ON TRIALS	ODD-EVEN r	SPEARMAN-BROWN r
30	U-maze A, errors	55	4–33	.921	.959
31	U-maze A, time	55	4–33	.926	.961
44	U-maze B, errors	49	2–12	.670	.803
45	U-maze B, time	49	2–12	.748	.856
42	Light discrimination, errors	50	1–20	.722	.839
43	Light discrimination, time	50	1–20	.854	.921

TABLE 20

Miscellaneous reliability coefficients

TEST NUMBER	NAME OF TEST	METHOD OF CALCULATING RELIABILITY r	N	r	SPEARMAN-BROWN r
32	Sand thirst	Subtests 1 + 2 vs. 3 + 4	52	.899	.947
4	Emotional defecation	Odd vs. even subtests (10 subtests)	55	.888	.941
37	Vaginal plugs	Plugs of test 5 vs. test 36	51	.662	.780

Reliability coefficients of the three remaining tests are presented in table 20. The reliability of the sand thirst test 32 was determined by correlating the scores on the first two subtests with those of the second two subtests rather than by correlating the scores on the odd-even subtests. Since the subtests of the sand thirst test were given two per day for two days, the coefficient as given in table 20 is probably more free of spurious correlation due to correlation of errors than would be the coefficient based on odd-even subtests. The reliability coefficient for the emotional defecation test 4 is based upon scores of 10 subtests and was obtained by correlating the scores on odd subtests with the scores on even subtests. Because of the limited range of scores obtained with the vaginal plug measure, the plug scores of the copulation tests 5 and 36 were combined to give the score for test 37. The reliability coefficient for this plug test 37 was obtained by correlating the vaginal plug score of test 5 with the vaginal plug score of test 36. Since tests 5 and 36 were separated by an interval of over 4 months, the uncorrected test-retest plug correlation of .66 is much higher than could be expected.

Summary: Of the 47 reliability coefficients presented, 2 are between .65 and .70; 5 are between .75 and .80; 7 are between .80 and .85; 11 are between .85 and .90; 16 are between .90 and .95; and 6 are over .95. Although higher reliability coefficients have been obtained in animal work, the reliabilities of the tests used in the present study are remarkably high when the character and length of the tests are considered.

D. *Intercorrelations*

Intercorrelations were obtained for a total of 47 measures. The number of tests thus makes impracticable the presentation of the coefficients of correlation in one large table. All intercorrelations will be presented, but in small tables convenient for presentation and discussion. All coefficients are Pearson product-moment r's calculated from grouped data. No correlations have been corrected for attenuation. With the exception of emotional defecation, test 4, a positive coefficient indicates a positive relation. The emotional defecation test has been scored as a test of emotion, and therefore a negative correlation with test 4 means that a small amount of defecation is associated with a "good" performance upon the other test being correlated. Standard errors for the correlations based upon the size of the group designated are presented in table 21. With the exception of correlations involving the activity measure, test 47, a correlation of .35 is approximately three times its standard error and will be spoken of as "significant."

The intercorrelations will be presented in two main topics, namely: 1. Intercorrelations between different measures of the same drive; 2. Intercorrelations between measures of one drive and measures of another drive.

1. *Intercorrelations between Different Measures of the Same Drive.* If a number of different tests of a given drive show consistent intercorrelations among themselves, some evidence would be obtained that individual differences in the strength of the drive are important in the determination of individual differences in tests involving that drive. If, for example, a number of different sex tests yield significant intercorrelations, then the data would indicate that individual differences in the performances on the sex tests were to some extent influenced by individual differences in the strength of the sex "drive" (the tendency to respond to the female), provided that the sex tests did not show equally high correlations with tests not involving sexual motivation. If, however, only low intercorrelations were obtained between the different sex tests, then the indication would be that individual differences in the performances on the sex tests were determined by factors other than the common sex motivation. In the present section, an examination of the intercorrelations between different tests of the same drive will be made with the special purpose of determining whether or not the given drive appears to be an important determiner of individual differences on the tests used.

In discussing the intercorrelations between different tests of the same drive, the table of intercorrelations for each drive will be considered under the following headings: (1) General trend. Under this heading the presence or absence of a general trend to correlation between the tests of the given drive will be noted. (2) Correlations between direct tests. Under this heading the intercorrelations between different direct tests of the given drive will be considered. Direct tests involve a measurement of the animal's direct response to the reward appropriate to the given drive. Eating tests, for example, are direct tests of the hunger drive. (3) Correlations of direct tests with indirect tests.

Indirect tests involve not so much the animals direct response to the reward, but rather a measurement of what the animal will do to get to the reward. Obstruction tests, for example, are indirect tests. (4) Correlations between indirect tests. The correlations between different indirect tests of a given drive will be discussed under this heading. (5) Trends of individual tests. Although a table may show no general trend to correlation it is possible that one or more tests may show consistent correlations with all other tests of the given drive; such trends will be noted under this heading.

In the case of some of the groups of tests, headings additional to the above five will be used where such use will aid in stressing certain points not covered by the above outline. The tables of intercorrelations between different measures of the same drive will be considered in the following order: a. Measures of the exploratory drive; b. Measures of the thirst drive; c. Measures of the hunger drive; d. Measures of the sex drive; and e. Measures of learning performance. While learning performance cannot be considered as a drive coordinate with the first four drives, the intercorrelations of the learning measures will, for convenience, be considered in the present section.

a. *Intercorrelations between different measures of the exploratory drive.* Table 22 presents the intercorrelations of the 7 tests designed to test exploratory behavior.

(1) General trend. The correlations of table 22 range from +.26 to +.65 and 16 of the 21 intercorrelations are .35 or over. The different exploratory tests thus show a definite and general trend to intercorrelation. The exploratory drive therefore appears to be a relatively important factor in the determination of individual differences on tests involving exploration. This interpretation is strengthened by the fact that the exploratory tests do not show such consistent trends to correlation with tests which do not primarily involve exploration (tables 27 to 31).

(2) Correlations between direct tests. The Dashiell maze test 1, the vertical maze test 2, the open field test 3, the sex barrier distance test 7, the open field food exploratory test 9, and the open field barrier exploratory test 11, are all more or less

direct tests of the exploratory drive. The animal is placed in a novel situation and his direct response to the situation in terms of the exploratory activity exhibited is recorded. All of these direct tests definitely tend to intercorrelate. The lowest correlations of the table under consideration include one or more of the three tests 7, 9, or 11. Each of these three tests involves motivation other than exploration, and is therefore somewhat less direct than tests 1, 2 and 3. Test 7, the sex barrier distance test, involves the presence of an inaccessible female in oestrum. The open field food exploratory test 9 involves hunger motivation plus a food reward in the center of the field. In the open field food barrier exploratory test 11, the animals were hungry, but a wire barrier prevented them from obtaining the food reward. Thus the addition of motivation other than exploration to an exploratory test tends to result in lower correlations with other exploratory tests, although the correlations may still be significant.

TABLE 21

Standard errors of coefficients of correlation for groups of the specified numbers

N		SIZE OF r:									
		.00	.10	.20	.30	.40	.50	.60	.70	.80	.90
51	Standard error of r	.140	.139	.134	.127	.118	.105	.090	.071	.050	.027
50	Standard error of r	.141	.140	.136	.129	.119	.106	.091	.072	.051	.027
49	Standard error of r	.143	.141	.137	.130	.120	.107	.091	.073	.051	.027
34	Standard error of r	.171	.170	.165	.156	.144	.129	.110	.087	.062	.033

TABLE 22

Intercorrelations between different measures of the exploratory drive ($N = 51$)

TEST NUMBER	NAME OF TEST	TESTS					
		2	3	7	9	11	41
1	Dashiell maze exploration	.509	.647	.594	.456	.397	.524
2	Vertical maze exploration		.490	.385	.308	.355	.367
3	Open field exploration			.496	.483	.651	.398
7	Sex barrier distance				.293	.384	.289
9	Open field food exploration					.337	.392
11	Open field barrier exploration						.263
41	Obstruction exploration						

(3) Correlations of direct tests with indirect tests. Only one indirect test, the obstruction exploratory test 41, was given. As the test numbers indicate, the obstruction exploratory test was given several months after the group of direct exploratory tests. In spite of this long temporal separation, the indirect test correlates significantly with four of the six direct tests. While the intercorrelations of the direct tests might possibly be interpreted as being due to the general similarity of the test situations and of the behavior required, their correlations with the indirect test make such an interpretation untenable. The highest correlation between the direct tests and the indirect exploratory test is that of .52 between the Dashiell maze test 1 and the obstruction test 41. It is of interest to note that the exploratory reward for the obstruction test consisted of a small Dashiell maze.

(4) Correlations between indirect tests. Since only one indirect test was given, no correlations between indirect tests were obtained.

(5) Trends of individual tests. Since all of the tests tend to correlate with all of the other exploratory tests, no additional discussion is necessary.

(6) Exploratory activity not an expression of general activity. Very low correlations between all the exploratory tests and general activity as measured by revolving drums (table 31) suggest that exploratory activity is independent of a more general drive to activity.

(7) Summary of intercorrelations between different measures of the exploratory drive. Each exploratory test shows definite trends to correlation with all other exploratory tests. The evidence therefore suggests that the exploratory tendency is of considerable importance in determining individual differences upon tests involving that drive, even when very different behavioral responses are required.

b. *Intercorrelations between different measures of the thirst drive.* Table 23 presents the intercorrelations of the 10 tests designed to measure the thirst drive.

(1) General trend. The correlations of table 23 range from

−.18 to +.83, but only 8 of the 45 intercorrelations are .35 or over. The majority of the correlations are under .20, and there is no general trend to correlation present.

(2) Correlations between direct tests. The drinking tests 14, 15, and 16 and the salt drinking tests 17 and 18 are classed as direct thirst tests. While the salt drinking tests involve an inhibition of drinking due to the salt in the water, they resemble the direct type of test more than they do the indirect tests. Within this group of five direct tests, five of the ten correlations are over .35 but four of the five significant correlations are between drinking time and amount scores. Since the amount and time scores were obtained from the same performance of the animals, they are not experimentally independent measures. The exception is the correlation of .36 between the drinking amount test 14 and the salt drinking amount test 17. This correlation is barely significant and all other correlations between the salt water tests and the tap water tests are negligible. There is thus no general trend to correlation between the direct thirst tests.

TABLE 23

Intercorrelations between different measures of the thirst drive (N = 51)

TEST NUMBER	NAME OF TEST	TESTS								
		15	16	17	18	19	32	33	34	35
14	Drinking amount	.514	.567	.361	.052	−.076	.318	.036	−.172	−.039
15	Drinking amt. per gm. body wt.		.631	.153	.190	.187	−.181	−.102	.069	−.018
16	Drinking time			.078	.116	.159	.149	−.050	.009	.002
17	Salt drinking amount				.832	.004	.281	.165	−.039	.018
18	Salt drinking time					.166	.158	.083	.088	−.006
19	Jump thirst						.124	.088	.086	.163
32	Sand thirst							.543	.262	.269
33	Speed thirst								.288	.414
34	Obstruction thirst, strong shock									.633
35	Obstruction thirst									

(3) Correlations of direct tests with indirect tests. The direct

thirst tests 14 to 18 inclusive yield no significant correlations with any of the indirect thirst tests, and show no tendency to correlate with the indirect measures. The correlation of .32 between drinking amount test 14 and sand thirst test 32 is probably to be attributed to the fact that both of these tests correlate with body weight (table 35). If body weight is partialled out by Yule's formula, the correlation drops to −.06.

(4) Correlations between indirect tests. The jump thirst test 19 shows no tendency to correlate with either the direct or indirect thirst tests. The sand thirst test 32, the speed thirst test 33, and the two obstruction thirst tests 34 and 35 show some tendency to intercorrelate with each other. Three of the six intercorrelations between these four tests are significant and the other three correlations are over .25. It is doubtful, however, that this trend to correlation can be attributed to the common thirst motivation, since these tests consistently show correlations quite as high with tests which do not involve thirst motivation (tables 32 to 35). The test numbers indicate that these four thirst tests were given relatively late in the test program, and it will be pointed out later that some factor favoring correlation appears to be related to lateness in the test program. The correlation of .63 between the two obstruction thirst tests 34 and 35 may be partially due to the identity of apparatus involved, but the fact that this correlation is higher than any other between obstruction tests (table 46) may indicate that the correlation is to be attributed in part to the identity of the drive involved. However, the close temporal proximity of the two obstruction thirst tests may also be a factor influencing the size of the correlation under consideration.

(5) Trends of individual tests. No one thirst test, direct or indirect, shows any marked tendency to correlate with all, or nearly all, of the thirst tests.

(6) Summary of intercorrelations of different measures of the thirst drive. The four indirect thirst tests occurring late in the test schedule show some tendency to intercorrelate, and there are some significant correlations within the group of direct thirst tests. On the whole, however, thirst tests do not tend to corre-

late with other thirst tests, and no thirst test shows any marked tendency to correlate with all other thirst tests.

c. *Intercorrelations between different measures of the hunger drive.* Table 24 presents the intercorrelations of the 14 tests designed to measure the hunger drive.

(1) General trend. The correlations of table 24 range from −.25 to +.91, but only 14 of the 91 intercorrelations are .35 or over. The majority of the correlations are under .20, and no general trend to intercorrelation is apparent.

(2) Correlations between direct tests. The open field food eating test 8, the eating tests 20 and 21, and the quinine eating tests 22 and 23 are classed as direct tests. The latter four tests yield four high intercorrelations but two of these significant correlations are between measures which are not experimentally independent. The open field food eating test 8 does not correlate significantly with any of the other four direct measures of eating behavior. Thus, within the group of five direct hunger tests there are some clusters of high correlations but the direct tests show no general trend to intercorrelation.

(3) Correlations of direct tests with indirect tests. The direct tests, as a group, do not tend to correlate with any of the indirect tests, and only 5 of 48 intercorrelations are .35 or over. Test 8, open field eating correlates .41 with the speed hunger slow trials test 28. Test 8 is a measure of the time the animal spends eating at a food can in the center of the open field. Hence, an animal which inhibits exploratory tendencies should make a higher score than one which does not. Test 28 is a measure of the speed of running to a food reward, but is limited to the 3 slowest of 5 trials. Hence, an animal which inhibits exploratory tendencies should have faster trials than one which does not and thus the correlation between tests 8 and 28 may be attributed to a common factor of inhibition of exploratory tendencies in a food situation. It might be expected that the exploratory tendencies which would lengthen the slow trials would be largely inhibited by most of the rats in their fastest 2 trials, and thus there would be a lower correlation between test 8 and the speed fast trials test 27 than between tests 8 and 28. The obtained correlation of .24 between

TABLE 24

Intercorrelations between different measures of the hunger drive (N = 51)

TEST NUMBER	NAME OF TEST	TESTS												
		10	20	21	22	23	12	24	25	26	27	28	13	29
8	Open field food eating	.178	−.208	−.248	−.037	.043	.215	−.072	−.071	−.018	.240	.413	−.017	.156
10	Open field food barrier		.006	.049	−.026	−.058	.024	−.028	−.015	−.078	−.035	−.205	.061	.116
20	Eating amount			.909	.530	.079	−.032	.347	.170	.272	.157	.087	.159	−.007
21	Eating amount per gm. body wt.				.561	.144	.093	.148	.018	.113	.154	.073	.299	.067
22	Quinine eating amount					.763	−.049	.028	−.023	−.008	.532	.382	.049	.037
23	Quinine eating time						−.064	−.172	−.033	−.168	.450	.323	−.035	−.046
12	Jump hunger							−.063	−.058	−.038	−.010	.075	.276	.213
24	Sand hunger digging time								.655	.908	.269	.235	.209	.207
25	Sand hunger idle time									.857	.147	.053	.131	.148
26	Sand hunger total time										.171	.121	.180	.210
27	Speed hunger, 2 fast trials											.829	.270	.187
28	Speed hunger, 3 slow trials												.204	.104
13	Obstruction no shock hunger													.349
29	Obstruction hunger													

tests 8 and 27 verifies this expectation. Test 20, eating amount, correlates .35 with the sand hunger digging test 24. Both of these tests correlate with body weight (table 38), and the correlation of tests 20 and 24 with body weight partialled out by Yule's formula is .02. The quinine eating tests 22 and 23 show fairly marked tendencies to correlate with the speed hunger tests 27 and 28. None of these four tests correlates significantly with body weight, and thus the correlations cannot be accounted for in terms of body weight. Furthermore, there appear to be no special conditions of the tests which can adequately account for the correlations. Hence, these correlations may be tentatively attributed to the common hunger drive factor.

(4) Correlations between indirect tests. The nine indirect hunger tests show no general trend to intercorrelate. Of the 28 correlations between these tests, only 5 are .35 or over. These 5 significant correlations can best be considered in three groups: *a.* Sand hunger tests. The three sand hunger tests, 24, 25 and 26 show high intercorrelations, but since test 26 is the sum of the scores of tests 24 and 25, the high correlations of test 26 with tests 24 and 25 involve spurious correlation and are of little interest. The sand digging time test 24 correlates .66 with the sand idle time test 25. Thus animals which dig through the sand in the least actual digging time spend the least time idling in the digging compartment, while the less efficient workers spend more time idling. *b.* Speed hunger tests. Another significant correlation in the group of hunger tests is that of .83 between the slow trials, test 28, and fast trials, test 27, of the speed hunger test. Thus the animal's place in the group with reference to speed of running can be determined from either the slow trials or the fast trials. The correlation is probably to be attributed to a speed or activity factor rather than to the common hunger drive. *c.* Obstruction hunger tests. The obstruction no shock hunger test 13 correlates .35 with the obstruction hunger test with shock test 29. The identity of the apparatus and handling techniques are probably adequate to account for this correlation, since these two tests show equally high correlations with obstruction tests not involving hunger motivation.

(5) Trends of individual tests. No hunger test shows any marked tendency to correlate with all, or nearly all, of the other hunger tests.

(6) Summary of intercorrelations between different measures of the hunger drive. The hunger tests do not show any general trend to correlation among themselves. There are a few limited clusters of correlations within both the groups of direct and indirect tests, but in most cases, these limited clusters of correlations can probably be accounted for best in terms of similarity of performance or in terms of factors other than the common hunger motivation.

TABLE 25

Intercorrelations between different measures of the sex drive (N = 51)

TEST NUMBER	NAME OF TEST	TESTS					
		36	37	6	38	39	40
5	Copulation	.662	.676	.573	.408	.380	.444
36	Copulation retest		.690	.483	.677	.603	.643
37	Vaginal plugs (of test 5 and 36)			.404	.434	.514	.402
6	Sex barrier time				.218	.228	.153
38	Sand sex					.639	.512
39	Speed sex						.523
40	Obstruction sex						

d. *Intercorrelations between different measures of the sex drive.* Table 25 presents the intercorrelations of the 7 tests designed to measure the sex drive.

(1) General trend. The correlations of table 25 range from +.15 to +.69; 18 of the 21 intercorrelations are over .35, and 11 of these are over .50. The different sex tests thus show a very general and very marked tendency to intercorrelate and the sex drive appears to be an important factor in the determination of individual differences upon very different tests involving sex motivation. This interpretation is strengthened by the fact that while the sex tests do tend to correlate with some non-sexual tests, in no case is the tendency as strong nor as consistent as is the tendency of the sex tests to correlate with other sex tests.

(2) Correlations between direct tests. Three direct tests of

the sex drive were obtained, namely, the copulation test 5, the copulation retest 36, and the vaginal plug score, test 37. These three tests yield correlations ranging from .66 to .69, thus showing considerable consistency in the rats' direct sexual responses to a female in oestrum. This consistency is really amazing in view of the fact that the two copulation tests 5 and 36 were separated by an interval of 127 days and yet yield a correlation of .66. And, as reported in the section on reliability, the vaginal plug score of test 5 correlates .66 with the vaginal plug score of test 36 (table 20). Thus both the copulation and vaginal plug scores are consistent even over a period of some months. That the two types of scores are not entirely independent is indicated by the correlations of .68 and .69 between plug scores and copulation scores. Since the plug score, test 37, is a combination of the plugs delivered on tests 5 and 36, the correlations between the plug score and either of the copulation scores are probably lower than they would be under optimum conditions. The writer (1) reported a correlation of .74 between the plug score of test 5 only and the copulation test 5.

(3) Correlations of direct tests with indirect tests. All of the direct sex tests correlate significantly with all of the indirect tests, and if the factor of temporal separation be taken into account the direct tests appear to be no more closely related to any one indirect test than to any other. In general, the copulation retest 36 yields higher correlations with the indirect tests than do the other two direct tests. These higher correlations are probably to be attributed to differences in temporal separation of the tests, a factor to be discussed in a succeeding section.

(4) Correlations between indirect tests. The sand sex test 38, the speed sex test 39, and the obstruction sex test 40 yield intercorrelations ranging from .51 to .64. It is possible that these correlations are to be attributed in part to some factor associated with late position in the testing program (see p. 104). The sex barrier time test 6 yields low correlations with the other three indirect sex tests. The low correlations of test 6 may be due in part to the fact that test 6 was given some months before the other indirect tests, but it is also possible that indirect tests may

be found which will yield low intercorrelations among themselves but which will all correlate significantly with the direct tests.

(5) Trends of individual tests. Because of the very general trend to correlation between the different sex tests no consideration need be given to the individual tests.

(6) The influence of temporal separation between tests upon the size of correlations. The copulation test 5 and the sex barrier time test 6 were given early in the testing program while the copulation retest 36, and the sand, speed and obstruction sex tests, 38, 39 and 40 were given about four months later. Since the two copulation tests vary in their temporal separation from the different indirect tests, some evidence of the effect of temporal separation upon correlations may be obtained by a consideration of the correlations of the copulation tests with the indirect tests. The three indirect tests which were given at about the same time as the copulation test 36 show correlations ranging from .60 to .68 with that test. The same three tests show correlations ranging from but .38 to .44 with the copulation test 5 given early in the testing program. The one indirect test, test 6, given at about the same time as copulation test 5 correlates .57 with test 5 and .48 with copulation test 36. Thus, assuming that the two copulation tests are comparable, an interval of about four months between a copulation test and an indirect sex test appears to lower the correlation between two such tests by from .09 to .27 correlation points.

(7) Summary of intercorrelations between different measures of the sex drive. Rats which copulate most frequently at one time also copulate most frequently several months later, they also deliver more vaginal plugs, spend more time near the female in the sex barrier test, take less time to dig through sand to get to a female, run faster to get to a female, and cross an electrified grid more frequently to get to a female, than do rats which copulate less frequently. There is also a very general trend for all of the sex tests to show significant correlations with all other sex tests, and the evidence therefore suggests that the sex drive is of considerable importance in determining individual differences upon tests involving sex motivation, even though the tests be markedly different in character.

e. *Intercorrelations between different measures of learning performance.* The intercorrelations between the 6 learning scores are presented in table 26.

(1) General trend. The correlations of table 26 range from +.08 to +.77; ten of the 15 intercorrelations are .35 or over, and there is a fairly general trend to correlation between all of the tests. The time scores tend to yield higher correlations than do the corresponding error scores.

(2) Intercorrelations of the four maze scores, tests 30, 31, 44 and 45. The time-error correlation of U-maze A is .76 and of U-maze B .62. These correlations fall within the expected range of time-error relations. The inter-maze correlations, however, are comparatively low, ranging from but .08 to .31. Any of a large number of factors may have been influential in producing these low inter-maze correlations. Only carefully controlled and comprehensive experimentation with mazes can aid in determining why some mazes show high intercorrelations and others show low intercorrelations.

TABLE 26

Intercorrelations between different measures of learning performance

TEST NUMBER	NAME OF TEST	N	TESTS				
			31	44	45	42	43
30	U-maze A, errors	51	.760	.313	.289	.358	.457
31	U-maze A, time	51		.081	.296	.466	.555
44	U-maze B, errors	49			.615	.307	.358
45	U-maze B, time	49				.464	.756
42	Light discrimination, errors	50					.774
43	Light discrimination, time	50					

(3) Intercorrelations between maze scores and light discrimination scores. Light discrimination errors, test 42, correlates .36 with U-maze A errors and .31 with U-maze B errors. In view of the low correlations between the two mazes of the present study, and the zero correlations between light discrimination and maze errors reported by Tomilin and Stone (12), these correlations are somewhat disturbing. The discrepancy between the

results reported by Tomilin and Stone and those of the present study may be due to such factors as differences in maze patterns, in distribution of trials, in previous and intervening experience, and possible differences in motivation. It also appears possible that long continued experience on a wide variety of tests tends to result in correlations that would not be obtained without such experience. The light discrimination time score, test 43, correlates significantly with all the maze measures, i.e. with both time and error scores of the two mazes, and light discrimination error scores correlate significantly with the maze time scores. Thus there is added evidence of a real relation between light discrimination and maze performance *under the conditions of this experiment.*

(4) Correlation between light discrimination scores. The light discrimination time score correlates .77 with the light discrimination error score. This correlation is approximately equivalent to correlations between maze time and error scores.

(5) Summary of intercorrelations between different measures of learning performance. The two mazes yield higher correlations with the light discrimination scores than they do with each other. There is a fairly general tendency for all of the learning measures to correlate with all other learning measures, the tendency being most marked in the case of the light discrimination time score.

2. *Intercorrelations between Measures of one Drive and Measures of Another Drive.*

This group of intercorrelations will be presented as follows: first the correlations between the group of exploratory tests with each of the other groups of tests (thirst, hunger, sex, learning and the miscellaneous tests 4, 46 and 47) will be presented. Then the correlations of the group of thirst tests with each of the remaining groups will be presented and so on. Since few of the tables show consistent trends to correlation between groups of tests, it is of some interest to determine whether or not any test of a given group tends to correlate with most of the tests of another group. In this way some evidence of interrelationship between measures of different drives may be obtained. In the discussion to follow, reliance will be placed upon two criteria of 'trend' or 'tendency' to correlation. A test will be described as showing a trend to correlation with a given group of tests if: (1) the signs of the coefficients are largely homogeneous (i.e. nearly all plus or all minus) and (2) most of the correlations between the given test and each of the group of tests are over .20 to .25. These are arbitrary and subjective criteria and have been adopted merely to simplify and standardize the following presentation.

a. *Intercorrelations of the group of exploratory tests with all other tests.*

(1) Intercorrelations between the group of exploratory tests and the group of thirst tests. The intercorrelations between these two groups of tests are presented in table 27. Of the 70 correlations, only 6 are over .35, and there appears to be no general trend to correlation between the two groups of tests.

(a) Intercorrelations of the individual thirst tests with the group of exploratory tests. Test 18, the salt drinking time test, shows a slight tendency to correlate with four of the seven exploratory tests. Test 19, jump thirst, yields correlations of over .20 with six of the seven exploratory tests, and two of the correlations are significant. These correlations are on the whole considerably higher than the correlations of test 19 with other thirst tests (table 23). Since the jump thirst test 19 was not the animal's first experience with the jump apparatus, the correlations with the exploratory tests cannot be accounted for in terms of the novelty of the test situation. On the basis of intercorrelations, the jump thirst test is a better measure of exploratory behavior than of thirst behavior, but there appears to be no reason why this should be so. Speed thirst, test 33, has five of seven correlations over .27 with the exploratory tests, and three of the correlations are significant. This trend to correlation is probably to be attributed to the speed aspect of test 33 rather than to the thirst aspect. Animals which tend to move rapidly when they move probably make higher scores on the exploratory tests than animals which tend to move leisurely; thus a correlation between speed and exploratory tests would be produced.

TABLE 27

Intercorrelations between the group of exploratory tests and the group of thirst tests (N = 51)

Test 14. Drinking amount
15. Drinking amt. per gm. wt.
16. Drinking time
17. Salt drinking amt.
18. Salt drinking time

Test 19. Jump thirst
32. Sand thirst
33. Speed thirst
34. Obstruction, strong shock
35. Obstruction thirst

TEST NUMBER	NAME OF TEST	TESTS									
		14	15	16	17	18	19	32	33	34	35
1	Dashiell maze exploration	−.195	.055	−.303	.082	.218	.348	.106	.142	.121	.051
2	Vertical maze exploration	−.152	.204	.022	.113	.313	.359	.156	.309	.249	.281
3	Open field exploration	−.038	−.089	−.260	.094	.158	.230	.288	.520	.051	.133
7	Sex barrier distance	.146	.218	−.128	.085	.076	.133	.086	.035	.101	.155
9	Open field food exploration	.181	.091	−.154	.297	.270	.247	.181	.274	−.227	−.146
11	Open field barrier exploration	.029	.151	−.055	.057	.141	.296	.184	.394	.220	.204
41	Obstruction exploration	−.017	.048	−.019	.181	.265	.231	.264	.428	.227	.385

The significant correlation of .39 between the obstruction thirst test 35 and the obstruction exploratory test 41 is probably to be attributed to a factor associated with the identity of the apparatus used in the two tests. The remaining thirst tests 14, 15, 16, 17, 32, 34 and 35 do not correlate with the exploratory tests.

(b) Intercorrelations of the individual exploratory tests with the group of thirst tests. None of the exploratory tests yield consistent trends to correlation with the entire group of thirst tests, but if the drinking tests, tests 14 to 18 inclusive, be omitted, there are several exploratory tests which show correlational trends with the remaining indirect thirst tests 19, 32, 33, 34 and 35. The vertical maze exploration test 2, the open field test 3, the open field barrier test 11, and the obstruction exploratory test 41, all show such trends in greater or lesser degree. On the whole it appears reasonable enough to suppose that the tendency of the rat to be active, i.e. explore, favors goodness of performance in a number of tests, and the correlations under consideration support such an assumption. The remaining exploratory tests 1, 7 and 9 show no tendency to correlation with the thirst tests.

(2) Intercorrelations between the group of exploratory tests and the group of hunger tests. Table 28 presents the intercorrelations between these two groups of tests. Of the 98 correlations in the table, only 7 are .35 or over, and the table shows no general trend to intercorrelation.

(a) Intercorrelations of the individual hunger tests with the group of exploratory tests. The sand hunger tests 24, 25 and 26 yield only slight, if any, trends to correlation with the exploratory tests. The small correlations between the idle time on the sand apparatus, test 25, and the exploratory tests indicate that the idle time on the sand test is probably not due to the animal's tendency to explore the digging compartment. The speed hunger tests 27 and 28 show some tendency to correlate with the exploratory tests, but the trend is more or less limited to the open field exploratory tests with and without barrier, tests 3 and 11, and the sex barrier distance test 7, and the obstruction exploratory test 41. Within this limited group of tests, three of the eight

TABLE 28

Intercorrelations between the group of exploratory tests and the group of hunger tests (N = 51)

Test 8. Open field food eating time
10. Open field food barrier
20. Eating amount
21. Eating amount per gm. body wt.
22. Quinine eating amount
23. Quinine eating time
12. Jump hunger

Test 24. Sand hunger digging time
25. Sand hunger idle time
26. Sand hunger total time
27. Speed hunger, 2 fast trials
28. Speed hunger, 3 slow trials
13. Obstruction no shock hunger
29. Obstruction hunger (with shock)

TEST NUMBER	NAME OF TEST	TESTS													
		8	10	20	21	22	23	12	24	25	26	27	28	13	29
1	Dashiell maze exploration	−.175	.033	−.052	.051	−.045	−.144	.079	.076	.084	.118	.133	.064	.291	.030
2	Vertical maze exploration	−.055	.298	−.224	−.074	−.159	−.157	.265	.119	.123	.124	.172	−.001	.229	.222
3	Open field exploration	−.071	−.018	.136	.167	.196	−.015	.102	.326	.158	.281	.461	.314	.312	.113
7	Sex barrier distance	.015	.041	−.099	−.096	.013	.002	.126	.128	.138	.135	.264	.216	.047	.078
9	Open field food exploration	−.801	−.064	.147	.162	.066	.014	−.091	.223	.073	.105	.078	−.110	.126	.048
11	Open field barrier exploration	.075	−.182	−.048	.026	.039	−.080	.222	.256	.074	.220	.366	.375	.382	.345
41	Obstruction exploration	−.233	.145	.010	.012	.017	−.037	−.056	.244	.234	.229	.310	.134	.425	.268

correlations are .37 or over. The interpretation of this correlational tendency in terms of a speed factor has already been suggested in a preceding section. The obstruction no shock hunger test 13 correlates with five of the exploratory tests and two of the correlations are significant. Since test 13 was the animals' first experience with the obstruction apparatus, it may be that performance on the test is considerably influenced by exploratory tendencies. The correlation of .43 between the no shock hunger test 13 and the obstruction exploratory test 36 may be partially due to the identity of apparatus in the two tests; but since this correlation is higher than the correlation between either of the two tests and any other obstruction test, it appears probable that the correlation is partly due to a common exploratory factor. On the whole, the no shock hunger test correlates higher with the exploratory tests than it does with hunger tests, and may therefore be considered a better test of exploratory behavior than of hunger. The remaining hunger tests 8, 10, 20, 21, 22, 23, 12 and 29 do not correlate with the exploratory tests. The correlation of −.80 between open field food eating time, test 8, and the open field food exploratory test 9 is due to the direct conflict between these two scores in the test situation. Both of the scores are based upon the animals' behavior in the open field when a food can is present in the center of the field. If the animal spends most of his time eating he will have little time left to spend in running about the open field, and hence a negative correlation will result.

(b) Intercorrelations of the individual exploratory tests with the group of hunger tests. None of the exploratory tests shows any marked tendency to correlate with all of the hunger tests, but if the group of eating tests be omitted, there are several fairly well marked trends with the remaining hunger tests 12, 24, 25, 26, 27, 28, 13 and 29. The open field exploratory test 3 yields consistent correlations with the hunger tests just listed, one of the correlations being significant. The open field barrier exploration test 11 shows a marked trend to correlation with the hunger tests under consideration, four of the eight correlations being .35 or over. Test 11 is thus a better test of hunger than are some of the hunger tests, but its correlations with other exploratory tests indicate that it also involves an exploratory factor. The experimental situation upon which test 11 is based presumably involves both exploratory and hunger drives. The hungry animal is placed in the open field, a food can is in the center but a wire barrier around the can prevents access to food. The score for test 11 consists of the number of marked off squares the animal traverses in sections other than the one immediately adjacent to the barrier. It was originally hoped that the time spent near the barrier, test 10, would provide a measure of the hunger drive, but test 10 failed to correlate significantly with any other hunger test. It appears that both hunger and exploratory motivation are involved in the barrier test, but instead of being expressed separately in the two scores taken, they are both involved in the one score of test 11. Test 11 is thus a complex test showing relation to both exploratory and hunger measures. The obstruction exploratory test 41 tends to correlate with the indirect hunger tests, although this trend is not so marked as in the case of the barrier exploration test 11. The one significant correlation, that with obstruction no shock hunger test 13, has been previously discussed. On the whole, the obstruction exploratory test 41 correlates as well with the hunger tests as do some of the hunger tests. The remaining exploratory tests 1, 2, 7, and 9 do not correlate consistently with even limited groups of the hunger tests.

(3) Intercorrelations between the group of exploratory tests and the group of sex tests. The intercorrelations between these two groups of tests are presented in table 29. Of the 49 correlations in the table, only three are .35 or over.

(a) Intercorrelations of the individual sex tests with the group of exploratory tests. The obstruction sex test 40 yields the highest and most consistent correlations with the exploratory tests, while the sand sex test 38 and the speed sex test 39 show less marked tendencies. The copulation tests 5 and 36 yield several correlations of .24 to .35 but there is no very general tendency to correlation. The vaginal plug test 37 and the sex barrier time test 6 fail to correlate with the exploratory tests.

(b) Intercorrelations of the individual exploratory tests with the group of sex tests. The open field exploratory test 3 yields the most consistent correlations with the sex tests, five of the seven correlations being over .30. A consistent but somewhat less marked trend is shown by the open field food exploration test 9. The sex barrier distance test 7 does not correlate with any sex test (table 29) but does correlate with different exploratory tests (table 22), while the sex barrier time test 6 correlates with the sex tests (table 25) but not with the exploratory tests (table 29). Thus both the exploratory and sex aspects of the barrier situation influenced the performance by *differentially* affecting the two scores derived from that situation. This result contrasts with that obtained on a barrier test involving hunger and exploration in which the two latter drives both influenced the *same* score (p. 72). The remaining exploratory tests 1, 2, 7, 11, and 41 do not correlate consistently with the sex tests as a group. The correlation of .39 between the obstruction exploratory test 41 and the obstruction sex test 40 is probably due to the identity of the apparatus and the temporal proximity of the two tests.

TABLE 29

Intercorrelations between the group of exploratory tests and the group of sex tests (*N = 51*)

Test 5. Copulation
36. Copulation retest
37. Vaginal plugs
6. Sex barrier time

Test 38. Sand sex
39. Speed sex
40. Obstruction sex

TEST NUMBER	NAME OF TEST	TESTS						
		5	36	37	6	38	39	40
1	Dashiell maze exploration	.204	.189	−.036	.064	.165	.206	.282
2	Vertical maze exploration	−.042	.137	−.228	−.039	.252	.169	.443
3	Open field exploration	.319	.354	.168	.238	.300	.342	.322
7	Sex barrier distance	.093	.048	−.152	−.214	.101	.000	.177
9	Open field food exploration	.256	.236	.161	.260	.289	.264	.278
11	Open field barrier exploration	.232	.265	.091	.085	.099	.223	.220
41	Obstruction exploration	.162	.198	−.107	−.023	.290	.323	.388

(4) Intercorrelations between the group of exploratory tests and the group of learning measures. The intercorrelations

between these two groups of tests are presented in table 30. Of the 42 correlations, only 3 are .35 or over, and there is no general trend to correlation between all exploratory tests and all measures of learning.

(a) Intercorrelations of the individual learning tests with the group of exploratory tests. The U-maze A time score, test 31, is the only learning measure which shows any tendency to correlate with the group of exploratory tests. Five of the seven correlations between test 31 and the exploratory measures range from .20 to .35. The positive signs mean that animals making high scores on the exploratory tests take *less* time in learning the maze. It is of some interest that the moderate tendency of the learning time scores to correlate with the exploratory tests is found only in the time score of the first learning test given, U-maze A. The light discrimination test was the second learning test given and only three of the correlations of the time score with exploratory tests are over .20. The time score correlations of the third learning test given, U-maze B, closely approximate zero. The only correlations of any size between the learning error scores and the exploratory tests are negative in sign. The negative correla-

TABLE 30

Intercorrelations between the group of exploratory tests and the group of learning measures

Test 30. U-maze A errors
31. U-maze A time
44. U-maze B errors

Test 45. U-maze B time
42. Light discrimination errors
43. Light discrimination time

TEST NUMBER	NAME OF TEST	TESTS					
		30	31	44	45	42	43
		N = 51		N = 49		N = 50	
1	Dashiell maze exploration	−.035	.207	−.166	.001	.049	.078
2	Vertical maze exploration	−.113	.213	−.115	.212	.116	.278
3	Open field exploration	.112	.350	−.098	.121	.068	.223
7	Sex barrier distance	−.125	.005	−.223	.023	−.055	.012
9	Open field food exploration	.010	.201	−.103	−.108	−.042	.039
11	Open field barrier exploration	.133	.335	−.344	.044	.131	.260
41	Obstruction exploration	−.366	−.045	−.350	−.110	−.051	.048

tions mean that rats making high exploratory scores tend to make *more* errors in the maze tests. The evidence thus indicates that a strong exploratory drive may impair the learning performance with reference to the error score, but under some conditions may tend to reduce the learning time. The failure of the light discrimination error score, test 42, to yield any correlations of even moderate size with the exploratory tests may possibly be due to the fact that, since the light discrimination pathway is changed each trial, the rat will be able, ultimately, to explore all parts of the apparatus without necessarily deviating from the true path.

TABLE 31

Intercorrelations between the group of exploratory tests and the emotional defecation, body weight and activity tests

Test 1. Dashiell maze exploration
2. Vertical maze exploration
3. Open field exploration
7. Sex barrier distance
Test 9. Open field food exploration
11. Open field barrier exploration
41. Obstruction exploration

TEST NUMBER	NAME OF TEST	N	TESTS						
			1	2	3	7	9	11	41
4	Emotional defecation	51	.021	.167	−.333	−.021	−.031	−.272	.050
46	Body weight	51	−.328	−.411	−.031	−.121	−.001	−.125	−.073
47	General activity	34	−.023	.106	.270	.014	−.115	.168	.031

(b) Intercorrelations of the individual exploratory tests with the group of learning measures. None of the exploratory tests yield correlational trends with the learning measures. Such relations as exist between these two situations have been discussed in the preceding paragraph.

(5) Intercorrelations between the group of exploratory tests and the emotional defecation, body weight and activity tests. The intercorrelations between these tests are presented in table 31. None of the three tests of this miscellaneous group shows a marked trend to correlate with the exploratory tests; but body weight, test 46, correlations with exploratory tests all have negative signs, and the correlation of −.41 between body weight and vertical maze exploration, test 2, is significant. The general

lack of correlation between the defecation score test 4 and the exploratory tests 1, 2, and 3 is of interest in that the defecation score is derived from the feces excreted during these three exploratory tests. However, the correlation between the open field test 3 and the defecation test 4 is −.33. While this correlation is not quite significant it may indicate that there is some tendency for "emotional" rats to be less active in the open field test. The lack of correlation between general activity test 47 and the exploratory tests is also of interest in that it indicates that activity in the exploratory situations is independent of the type of activity measured by the activity drum, and that exploratory activity is not merely one form of a general activity. However, the reduced number of cases in all correlations involving the activity measure render any statements concerning this measure very tentative.

(6) Summary of trends to correlation of the individual exploratory tests with all other tests. *Test 1*, Dashiell maze exploration: no tendency to correlation with any group of tests. *Test 2*, vertical maze exploration: some trends to correlation with the group of thirst tests (exclusive of drinking tests) and a negative correlation with body weight. *Test 3*, open field exploration: limited trend to correlation with thirst tests (exclusive of drinking tests), hunger tests (exclusive of eating tests), and quite consistent correlations with sex tests. *Test 7*, sex barrier distance: no trends to correlation. *Test 9*, open field food exploration: a moderate trend to correlation with sex tests and a suggestion of correlation with some thirst tests. *Test 11*, open field barrier exploration: tendency to correlate moderately with thirst tests (exclusive of drinking tests), and with hunger tests (exclusive of eating tests). *Test 41*, obstruction exploration: correlations with thirst tests (exclusive of drinking tests), hunger tests (exclusive of eating tests) and negative correlations with maze errors.

b. *Intercorrelations of the group of thirst tests with other groups of tests.* The procedure followed in presenting the data for the exploratory tests will be followed in the present section.

(1) Intercorrelations between the group of thirst tests and the

TABLE 32

Intercorrelations between the group of thirst tests and the group of hunger tests (N = 51)

Test 8. Open field food eating time
10. Open field food barrier time
20. Eating amount
21. Eating amt. per gm. body wt.
22. Quinine eating amount
23. Quinine eating time
12. Jump hunger

Test 24. Sand hunger digging time
25. Sand hunger idle time
26. Sand hunger total time
27. Speed hunger fast trials
28. Speed hunger slow trials
13. Obstruction no shock hunger
29. Obstruction hunger with shock

TEST NUMBER	NAME OF TEST	TESTS													
		8	10	20	21	22	23	12	24	25	26	27	28	13	29
14	Drinking amount	−.179	−.091	.205	−.048	.103	−.014	−.100	.337	.215	.243	.110	.000	−.100	.275
15	Drinking amt. per gm. body wt.	−.006	−.043	−.320	−.264	−.034	.063	.173	−.210	−.167	−.215	.109	.047	−.005	.374
16	Drinking time	.126	.070	−.176	−.280	−.076	.053	.057	.094	.049	.057	.041	−.047	−.223	.269
17	Salt drinking amount	−.378	−.072	.079	.009	−.010	−.011	−.103	.147	−.021	.063	−.142	−.153	.080	.120
18	Salt drinking time	−.303	−.010	−.178	−.163	−.164	−.051	−.040	.038	−.181	−.085	−.117	−.101	.097	.074
19	Jump thirst	−.100	.266	.044	.173	−.011	.030	.570	.185	.071	.189	−.040	−.118	.311	.226
32	Sand thirst	−.137	−.063	.365	.170	.072	−.092	−.063	.844	.647	.794	.229	.133	.197	.096
33	Speed thirst	−.051	−.059	.250	.244	.311	.168	.053	.406	.264	.344	.565	.376	.384	.188
34	Obstruction thirst strong shock	.315	.169	−.268	−.197	−.221	−.154	.066	.193	.156	.250	.177	.210	.319	.291
35	Obstruction thirst	.326	.310	−.053	−.018	−.036	−.003	−.001	.339	.209	.303	.299	.226	.308	.490

group of hunger tests. The intercorrelations between these two groups of tests are presented in table 32. Of the 140 correlations of the table, only 12 are .35 or over.

(a) Intercorrelations of the individual hunger tests with the group of thirst tests. If the drinking tests 14 to 18 inclusive be omitted from consideration, the sand hunger tests 24, 25 and 26, the speed hunger tests 27 and 28, and the obstruction hunger tests 13 and 29 show more or less tendency to correlate with the remaining thirst tests. Seven of the highest correlations in table 32 may be attributed, at least in part, to the identity of the apparatus involved in giving the tests. These seven correlations are: that between the jump hunger and jump thirst tests, the three between the sand hunger and the sand thirst tests, two correlations between the speed hunger tests and the speed thirst tests, and the correlation between obstruction hunger and obstruction thirst. The obstruction hunger test 29 yields some trend to correlation with the direct drinking tests. The hunger tests 8, 10, 20, 21, 22, 23 and 12 show only sporadic correlations with the thirst tests.

(b) Intercorrelations of the individual thirst tests with the group of hunger tests. The speed thirst test 33 shows a fairly general trend to correlation with the group of hunger tests, a trend which becomes more marked if the eating tests be omitted from consideration. In fact, the speed thirst test correlates better with the hunger tests than with other thirst tests (table 23). The obstruction thirst test 35 also tends to correlate with the indirect hunger tests, showing a better trend to correlation with these tests than with other thirst tests. The remaining thirst tests yield only somewhat sporadic correlations with the hunger tests, and most of the significant correlations appear to be due to factors dependent upon the apparatus used.

(2) Intercorrelations between the group of thirst tests and the group of sex tests. Table 33 presents the intercorrelations between these two groups of tests. Of the 70 correlations of the table, 15 are .35 or over.

(a) Intercorrelations of the individual sex tests with the group of thirst tests. Only the obstruction sex test 40 shows a consistent tendency to correlate with the thirst tests, whether or not the drinking tests be taken into consideration. Although test 40 shows three negative correlations with drinking tests, the remaining seven correlations are all over .26 and five of the correlations are over .35. The obstruction sex test is thus a more consistent measure of thirst than any of the thirst tests (table 23) but at the same time it correlates well with other sex tests (table 25).

TABLE 33

Intercorrelations between the group of thirst tests and the group of sex tests (N = 51)

Test 5. Copulation
36. Copulation retest
37. Vaginal plugs
6. Sex barrier time
Test 38. Sand sex
39. Speed sex
40. Obstruction sex

TEST NUMBER	NAME OF TEST	TESTS						
		5	36	37	6	38	39	40
14	Drinking amount	−.041	.064	.127	−.061	.331	.262	−.090
15	Drinking amt. per gm. body wt.	−.377	−.246	−.350	−.286	−.137	.006	−.180
16	Drinking time	−.475	−.246	−.286	−.331	.080	.038	−.230
17	Salt drinking amount	.087	.167	.191	−.114	.290	.225	.362
18	Salt drinking time	−.104	.031	−.010	−.142	.168	.153	.335
19	Jump thirst	.035	.126	−.007	−.013	.225	.159	.263
32	Sand thirst	.192	.533	.368	−.036	.810	.591	.524
33	Speed thirst	.261	.439	.172	.150	.471	.637	.452
34	Obstruction thirst strong shock	.058	.130	−.064	−.006	.121	.079	.337
35	Obstruction thirst	.144	.136	−.054	−.071	.120	.159	.497

(b) Intercorrelations of the individual thirst tests with the group of sex tests. The sand thirst test 32 shows a marked tendency to correlation with sex tests, five of the seven correlations being .37 or over. Identity of apparatus and common correlations with body weight are probably adequate to account for most, but not all, of these high correlations. The position in the testing program may also be a factor affecting the correlations. The speed thirst test 33 also tends to correlate with the sex tests, especially with those occurring late in the test series, and thus the influence of position in the test series is indicated. Both the sand and speed thirst tests correlate better with the sex tests than they do with other thirst tests. The drinking tests 15 and 16 correlate with the direct sex tests 5, 36, and 37, and with the indirect sex barrier test 6, but the signs are consistently negative. The remaining thirst tests 14, 17, 18, 19, 34 and 35 yield only a few correlations of moderate size. The correlations of .34 and .50 between the obstruction thirst tests 34 and 35 and the obstruction sex test 40 are probably due to the identity of the apparatus involved.

TABLE 34

Intercorrelations between the group of thirst tests and the group of learning measures

Test 30. U-maze A errors
31. U-maze A time
44. U-maze B errors
Test 45. U-maze B time
42. Light discrimination errors
43. Light discrimination time

TEST NUMBER	NAME OF TEST	TESTS					
		30	31	44	45	42	43
		N = 51		N = 49		N = 50	
14	Drinking amount	−.125	−.330	.089	−.035	−.152	−.221
15	Drinking amt. per gm. wt.	−.200	−.206	−.106	.082	−.258	−.217
16	Drinking time	−.386	−.409	−.113	.003	−.143	−.195
17	Salt drinking amount	−.219	−.162	.017	.076	.043	−.005
18	Salt drinking time	−.292	−.113	−.205	.017	−.009	−.016
19	Jump thirst	−.011	.133	−.086	.012	.118	.165
32	Sand thirst	.090	.157	−.016	.221	.333	.398
33	Speed thirst	.184	.356	.025	.334	.254	.450
34	Obstruction thirst strong shock	.084	.144	−.114	.161	.278	.294
35	Obstruction thirst	−.032	.136	.016	.195	.270	.371

(3) Intercorrelations between the group of thirst tests and the group of learning measures. The correlations between these two groups of tests are presented in table 34. Of the 60 correlations in the table, only 7 are .35 or over.

(a) Intercorrelations of the individual learning tests with the group of thirst tests. None of the learning measures tends to correlate with all of the thirst tests, but there is some tendency for the U-maze A error and time scores, tests 30 and 31, to correlate negatively with the drinking tests. The light discrimination scores 42 and 43 correlate with those thirst tests occurring relatively late in the testing program, tests 32 to 35 inclusive.

(b) Intercorrelations of the individual thirst tests with the group of learning measures. The speed thirst test 33 correlates positively with the learning time tests 31, 45 and 43, thus indicating that a fast runner on the speed thirst test tends to take less time on the maze. This may appear to indicate that speed is one component in learning time (Tolman and Nyswander, 11) but the failure of the speed hunger test (table 38) to correlate with learning time scores casts some doubt upon this generalization. The drinking tests, particularly tests 15 and 16, give some indication of a negative relation with learning measures.

TABLE 35

Intercorrelations between the group of thirst tests and the emotional defecation, body weight and activity tests

Test 14. Drinking amount
15. Drinking amount per wt.
16. Drinking time
17. Salt drinking amount
18. Salt drinking time
19. Jump thirst

Test 32. Sand thirst
33. Speed thirst
34. Obstruction thirst, strong shock
35. Obstruction thirst

TEST NUMBER	NAME OF TEST	N	TESTS									
			14	15	16	17	18	19	32	33	34	35
4	Emotional defecation	51	−.060	.203	.262	−.018	.095	−.016	−.292	−.313	.025	−.095
46	Body weight	51	.665	−.232	.153	.189	−.184	−.258	.531	.130	−.245	−.002
47	Activity	34	−.087	−.285	−.129	−.181	−.145	−.033	.315	.415	.049	−.042

(4) Intercorrelations between the group of thirst tests and the emotional defecation, body weight, and activity tests. Table 35 presents the intercorrelations between the thirst tests and the three miscellaneous tests 4, 46 and 47. Of the 30 correlations in the table, only three are over .35. None of these miscellaneous tests shows any general trend to correlation with the thirst tests. Body weight, test 46, correlates .67 with amount of water drunk, test 14, and fails to correlate with amount of salt water drunk. Body weight also correlates .53 with the sand thirst test 33, indicating that individual differences in performance on

the sand test are conditioned to quite an extent by body weight. Heavy rats are presumably stronger than light weight rats and are thus able to dig through the sand in a shorter time than rats of less body weight. Activity, test 47, correlates .42 with the speed thirst test 33.

(5) Summary of trends to correlation of the individual thirst tests with all other tests. *Test 14*, drinking amount: correlates only with body weight. *Test 15*, drinking amount per gram body weight, and *test 16*, drinking time: tend to correlate negatively with direct sex tests and negatively with some learning measures. *Test 17*, and *test 18*, salt drinking amount and time: no tendencies to correlate with any group of tests. *Test 19*, jump thirst: tends to correlate with exploratory tests only. *Test 32*, sand thirst: tends to correlate with sex tests and with body weight. *Test 33*, speed thirst: tends to correlate with exploratory, hunger and sex tests, with learning time scores, and with activity. *Test 34*, obstruction thirst strong shock: no correlational trends. *Test 35*, obstruction thirst: tends to correlate with hunger tests other than the eating tests.

c. *Intercorrelations of the group of hunger tests with other tests.*

(1) Intercorrelations between the group of hunger tests and the group of sex tests. Table 36 presents the correlations between these two groups of tests. Of the 98 correlations in the table, 21 are .35 or over.

(a) Intercorrelations of the individual sex tests with the group of hunger tests. While none of the sex tests shows a tendency to correlate with the hunger tests as a group, most of the sex tests yield significant correlations with some of the hunger tests. Since these correlations consistently appear with certain of the hunger tests, they will be considered in the next paragraph.

(b) Intercorrelations of the individual hunger tests with the group of sex tests. The eating amount tests 20 and 21 show substantial and consistent trends to correlation with the sex tests. Eight of the 14 correlations between these two eating scores and the sex tests are .35 or over. Since the eating tests and some of the sex tests show substantial correlations with body weight, the correlations between these tests with the influence of body weight partialled out by Yule's formula are presented in table 37.

TABLE 36

Intercorrelations between the group of hunger tests and the group of sex tests (N = 51)

Test 5. Copulation
36. Copulation retest
37. Vaginal plugs
6. Sex barrier time
Test 38. Sand sex
39. Speed sex
40. Obstruction sex

TEST NUMBER	NAME OF TEST	TESTS						
		5	36	37	6	38	39	40
8	Open field food eating	−.194	−.200	−.221	−.117	−.292	−.213	−.184
10	Open field food barrier	−.118	−.107	−.265	.091	.000	−.214	.109
20	Eating amount	.435	.366	.478	.249	.463	.413	.124
21	Eating amt. per gm. body wt.	.447	.308	.400	.256	.314	.353	.135
22	Quinine eating amount	.217	−.016	.125	−.038	.102	.113	−.084
23	Quinine eating time	−.037	−.242	−.116	−.184	−.131	−.017	−.120
12	Jump hunger	.047	.082	.014	.058	.017	.159	.109
24	Sand hunger digging time	.227	.481	.350	.101	.734	.437	.445
25	Sand hunger idle time	.204	.335	.215	−.024	.573	.274	.304
26	Sand hunger total time	.277	.457	.360	.010	.681	.356	.384
27	Speed hunger fast trials	.153	.110	−.064	.066	.198	.272	.044
28	Speed hunger slow trials	.063	.047	−.039	.010	.062	.168	−.010
13	Obstruction no shock hunger	.324	.428	.170	.212	.341	.384	.324
29	Obstruction hunger (with shock)	.189	.137	.121	.016	.065	.147	.296

TABLE 37

Intercorrelations between certain hunger tests and the group of sex tests with body weight, test 47, partialled out by Yule's formula

Test 5. Copulation
36. Copulation retest
37. Vaginal plugs
6. Sex barrier time
Test 38. Sand sex
39. Speed sex
40. Obstruction sex

TEST NUMBER	NAME OF TEST	TESTS						
		5	36	37	6	38	39	40
20	Eating amount	.420	.257	.317	.229	.260	.288	.138
21	Eating amt. per gm. body wt.	.424	.257	.339	.238	.233	.298	.134
24	Sand hunger digging time	.160	.406	.154	.046	.643	.323	.525
25	Sand hunger idle time	.151	.254	.062	−.074	.478	.167	.321
26	Sand hunger total time	.226	.383	.206	−.048	.591	.243	.423
13	Obstruction no shock hunger	.344	.472	.225	.223	.432	.437	.327

While the correlations of table 37 are somewhat smaller than the original correlations of table 36, the two eating tests still show consistent and fairly substantial trends to correlation with the sex tests. All of the sand hunger tests 24, 25 and 26 tend to correlate with the sex tests. Since these hunger tests also correlate with body weight, their correlations with the sex tests with body weight partialled out are also presented in table 37. The trend to correlation of the sand hunger tests with the sex tests are still present in table 37, but are less broad and less marked than in the original correlation table 36. The obstruction no shock hunger test 13 also shows relatively high correlations with the sex tests. In an earlier study (2) the writer reported a correlation of .32 between a copulation test and an obstruction no shock hunger test and a correlation of .39 between copulation and the no shock hunger retest. The no shock hunger test of the present study is more nearly comparable to the retest of the earlier study, and the results for the two studies are thus in excellent agreement with reference to the correlations under consideration. Since the exploratory tests tend to correlate with the no shock hunger test of the present study and also with some of the sex tests, the correlations may be due to some exploratory factor. That the correlations are not due to body weight is shown by the fact that the correlations actually increase slightly when body weight is partialled out (table 37). The hunger tests which have just been discussed, test 20, 21, 24, 25, 26 and 13, all show much more substantial and consistent trends to correlation with the group of sex tests than they do with the group of hunger tests. On the basis of the correlations obtained, they are much better sex tests than they are hunger tests. The remaining eight hunger tests do not yield correlational trends with the sex tests.

(2) Intercorrelations between the group of hunger tests and the group of learning measures. The intercorrelations between these two groups of tests are presented in table 38A. Of the 84 correlations, only 2 are .35 or over. Although hunger was used as the motivation in all the learning tests, there are no trends to correlation of the individual learning measures with the group of hunger tests, nor of the individual hunger tests with the group of

TABLE 38

A. Intercorrelations between the group of hunger tests and the group of learning measures
B. Intercorrelations between the group of hunger tests and the emotional defecation, body weight and activity tests

A

Test 30. U-maze A errors
31. U-maze time
44. U-maze B errors
45. U-maze B time
42. Light discrimination errors
43. Light discrimination time

B

Test 4. Emotional defecation
46. Body weight
47. Activity

TEST NUMBER	NAME OF TEST	A						B		
		Tests								
		30	31	44	45	42	43	4	46	47
		N = 51		N = 49		N = 50		N = 51		N = 34
8	Open field food eating	.030	−.006	.026	.242	.149	.148	−.093	−.100	.135
10	Open field food barrier	−.186	−.061	.082	.070	.047	.050	.167	−.073	−.137
20	Eating amount	.121	.081	.337	.104	−.156	.142	−.281	.592	.303
21	Eating amt. per gm. body wt.	.171	.213	.343	.174	.214	.250	−.240	.244	.341
22	Quinine eating amount	−.061	−.066	.092	.068	−.152	−.012	−.269	.199	.277
23	Quinine eating time	−.132	−.131	−.072	−.135	−.184	−.136	−.133	−.047	.050
12	Jump hunger	.172	.132	.032	.238	.154	.271	−.265	−.255	.333
24	Sand hunger digging time	.110	.161	−.051	.109	.248	.354	−.258	.567	.174
25	Sand hunger idle time	.121	.101	−.029	−.005	.218	.222	−.145	.394	.066
26	Sand hunger total time	.181	.202	−.050	.120	.303	.372	−.246	.469	.168
27	Speed hunger fast trials	−.137	.026	−.198	.090	−.014	.092	−.277	.101	.259
28	Speed hunger slow trials	.025	.030	−.153	.124	.051	.124	−.269	−.001	.299
13	Obstruction no shock hunger	.099	.253	−.233	−.032	.170	.203	−.211	−.083	.238
29	Obstruction hunger (with shock)	−.072	−.003	.019	.202	.115	.260	−.135	.022	−.157

learning measures. However, light discrimination time, test 43, shows a slight tendency to correlate with the hunger tests, exclusive of the eating tests, the two significant correlations of the table being between test 43 and the sand hunger tests 24 and 26.

(3) Intercorrelations between the group of hunger tests and the emotional defecation, body weight and activity tests. The intercorrelations between these tests are presented in table 38B. Emotional defecation test 4 correlates negatively with the hunger tests, but the correlations are very moderate in size. The negative sign indicates that animals making 'good' scores on the hunger tests tend to defecate least on the defecation tests. Body weight, test 46, correlates significantly with the amount of food eaten, test 20, and with the sand hunger tests 24, 25 and 26. The activity test 47 yields moderate correlations with several of the hunger tests, but the small size of the group (N = 34) on the activity test makes the significance of such moderate correlations very doubtful.

(4) Summary of trends to correlation of the individual hunger tests with all other tests. *Test 13,* obstruction no shock hunger: tends to correlate with exploratory tests, thirst tests (other than drinking tests), and with the sex tests. *Test 20,* eating amount: correlates with the group of sex tests and with body weight. *Test 21,* eating amount per gram body weight: correlates with the group of sex tests. *Tests 24, 25 and 26,* sand hunger tests: tend to correlate with thirst tests (other than drinking tests), with sex tests, and with body weight. *Tests 27 and 28,* speed hunger tests: slight trend to correlation with the exploratory tests. *Test 8,* open field food eating; *test 10,* open field food barrier; *test 12,* jump hunger; *test 22,* quinine eating amount; *test 23,* quinine eating time; and *test 29,* obstruction hunger: yield no correlational trends with other tests.

d. *Intercorrelations of the group of sex tests with other tests.*

(1) Intercorrelations between the group of sex tests and the group of learning measures. The correlations between these two groups of tests are presented in table 39A. Of the 42 correlations of the table, 16 are .35 or over.

(a) Intercorrelations of the individual learning scores with the group of sex tests. All of the learning measures tend to correlate with the copulation tests 5, 36 and 37, and also show some tendency to correlate with most of the indirect sex tests. This tendency is most marked in the case of the light discrimination time score, and is least substantial in the case of the U-maze A error and time scores. Some of the correlations may be due to placement in the test series but correlations with the original copulation test 5 cannot be accounted for in this manner. It is to be noted, however, that the correlations with the copulation retest are uniformly higher than the correlations with the original copulation test.

TABLE 39

A. Intercorrelations between the group of sex tests and the group of learning measures
B. Intercorrelations between the group of sex tests and the emotional defecation, body weight and activity tests

A

Test 30. U-maze A errors
31. U-maze A time
44. U-maze B errors
45. U-maze B time
42. Light discrimination errors
43. Light discrimination time

B

Test 4. Emotional defecation
46. Body weight
47. Activity

TEST NUMBER	NAME OF TEST	A						B		
		Tests								
		30	31	44	45	42	43	4	46	47
		N = 51		N = 49		N = 50		N = 51		N = 34
5	Copulation	.159	.120	.347	.254	.350	.383	−.437	.171	.316
36	Copulation retest	.285	.330	.389	.473	.418	.559	−.402	.283	.362
37	Vaginal plugs	.367	.265	.458	.340	.407	.404	−.496	.414	.270
6	Sex barrier time	.234	.141	.399	.149	.213	.230	−.224	.111	.074
38	Sand sex	.005	.047	.120	.244	.262	.350	−.203	.469	.356
39	Speed sex	.205	.250	.181	.290	.273	.369	−.423	.327	.552
40	Obstruction sex	.072	.331	.167	.349	.342	.516	−.325	.022	.240

(b) Intercorrelations of the individual sex tests with the learning measures. The copulation retest 36, and the vaginal plug score, test 37, show the most substantial trends to correlation with the learning measures, eight of the twelve correlations being significant. Of the remaining sex tests, all but the sand sex test

38 show fairly marked trends to correlation with the learning measures. In an earlier study (2), the writer reported a correlation of .77 between maze errors and copulation and one of .66 between maze time and copulation. While these correlations are higher than any of the copulation-learning correlations of the present study, the correlations of table 39 clearly confirm the existence of a relation between copulation and learning scores. Other unpublished studies of the writer indicate that the correlation may fail to appear under some conditions. In the present study, the copulation test 5 correlates only .16 and .12 respectively with U-maze A errors and time, thus indicating that the copulation-learning relation is not to be expected under all conditions. As a paper dealing with the copulation-learning relationship is in preparation, no further treatment will be given to this topic in the present paper. It should be noted, however, that hunger was used as the motivation for all of the learning tests, and that no female rats were run on the mazes or discrimination box while the males were being tested.

(2) Intercorrelations between the group of sex tests and the emotional defecation, body weight and activity tests. This group of correlations is presented in table 39B. Test 4, emotional defecation correlates negatively with the sex tests, and the correlations with the copulatory measures, tests 5, 36 and 37, are all over −.40. Thus high frequency of copulation and plugs, and, in general, "good" performance on all of the sex tests, is associated with a small amount of defecation in test 4. There is a possibility that the negative correlation may be due to endocrine factors. The writer is attempting to test this possibility at the present time, and feels that any theory designed to explain the negative relation between copulation and emotional defecation would be premature and inadequate. Body weight, test 46, correlates .41 with the number of vaginal plugs delivered, test 37, indicating that general bodily vigor as indicated by weight may be a factor in sexual vigor. Body weight also correlates .47 with the sand sex test 38 and shows some tendency to correlate with the copulation retest and the speed sex test. Activity test 47 shows a somewhat general tendency to correlate with the sex

tests, but because of the limited number of cases involved in the correlations with the activity test, only the correlation of .55 with the speed sex test 39 is significant. Activity correlates .32 and .36 respectively with the copulation tests 5 and 36. While these correlations are not significant, they are suggestive of a relation between copulation and activity tests. Anderson (2) reported correlations of .24 to .34 between copulation scores and activity, while Stone and Barker (9) reported a correlation of .177 between two such tests.

(3) Summary of trends to correlation of the individual sex tests with all other groups of tests. *Tests 5 and 36*, copulation tests: show somewhat doubtful tendencies to correlate with the group of exploratory tests, definite trends to correlation with learning measures, and significant negative correlations with emotional defecation. *Test 37*, vaginal plugs: correlates with the group of learning measures, with body weight, and negatively with emotional defecation. *Test 6*, sex barrier time: correlates moderately with learning measures. *Test 38*, sand sex: shows some slight tendency to correlate with exploratory tests, and a significant correlation with body weight. *Test 39*, speed sex: some tendency to correlate with exploratory tests, with the learning measures, with the activity test, and a significant negative correlation with emotional defecation. *Test 40*, obstruction sex: tends to correlate with the exploratory tests, the thirst tests (exclusive of drinking tests), and with the learning measures.

e. *Intercorrelations of the group of learning measures with other tests.*

(1) Intercorrelations between the group of learning measures and the emotional defecation, body weight and activity tests. These correlations are presented in table 40. Of the 18 correlations in the table, only three are over .35. Emotional defecation test 4 tends to correlate negatively with the learning measures, three of the six negative correlations being significant. The sign of the correlations means that fewness of errors and short learning time are associated with a small amount of defecation on the defecation test. Since the defecation test also correlates negatively with the sex tests, it may appear that the relation between

learning measures and sex tests previously mentioned is due to an emotional factor. It is to be noted, however, that the U-maze A error and time scores show lower correlations with sex tests than do the other learning measures (table 39), but the maze A scores give the highest negative relation with the defecation test. To determine the possible rôle played by emotion, as measured by the defecation test, in producing the correlations between sex tests and learning measures, table 41 presents the correlations between the copulation tests and the learning measures with emotional defecation, test 4, partialled out by Yule's formula. As table 41 shows, the correlations between the U-maze A scores and the direct sex tests become quite negligible when emotional defecation is partialled out, but substantial correlations are still present between the direct sex tests and the other learning measures.

TABLE 40

Intercorrelations between the group of learning measures and the emotional defecation, body weight and activity tests

Test 30. U-maze A errors
31. U-maze A time
44. U-maze B errors
Test 45. U-maze B time
42. Light discrimination errors
43. Light discrimination time

TEST NUMBER	NAME OF TEST	N	TESTS					
			30	31	44	45	42	43
4	Emotional defecation	51	−.403	−.421	−.077	−.261	−.282	−.398
46	Body weight	51	.069	−.145	.246	−.016	.059	−.017
47	Activity	34	.245	.248	−.034	.276	.288	.332

TABLE 41

Intercorrelations between the direct sex tests and the measures of learning with emotion, test 4, partialled out by Yule's formula

Test 30. U-maze A errors
31. U-maze A time
44. U-maze B errors
Test 45. U-maze B time
42. Light discrimination errors
43. Light discrimination time

TEST NUMBER	NAME OF TEST	TESTS					
		30	31	44	45	42	43
5	Copulation	−.021	−.078	.349	.161	.263	.253
36	Copulation retest	.147	.194	.392	.416	.347	.475
37	Vaginal plugs	.210	.071	.485	.251	.321	.259

It is to be noted that the size of correlations between the emotional defecation test and the learning measures vary with the position of the learning tests in the testing program. The first learning test (U-maze A) yields the highest correlations with the defecation test, the second learning test given (light discrimination) shows lower correlations; and the correlations with the defecation test tend to disappear in the case of the last learning test given (U-maze B). The evidence thus indicates that emotional factors tend to play a less important rôle in learning performance as the animals become more familiar with various learning situations. Body weight, test 46, shows no tendency to correlate with the learning measures. Activity test 47 correlates between .25 and .33 with five of the six learning measures, but the size of the group on the activity test renders this trend inconclusive.

(2) Summary of trends to correlation of the individual learning measures with all other groups of tests. *Test 30*, U-maze A errors: some tendency to correlate with sex tests and negatively with emotional defecation and drinking tests. *Test 31*, U-maze A time: shows a slight tendency to correlate with exploratory and sex tests and negative correlations with emotional defecation and drinking tests. *Test 44*, U-maze B errors: shows a slight tendency to correlate negatively with exploratory tests, and a marked tendency to positive correlation with sex tests, especially with the copulation scores. *Test 45*, U-maze B time: tends to correlate with the sex tests. *Test 42*, light discrimination errors: yields moderate correlations with thirst tests (exclusive of drinking tests), and tends to correlate with the sex tests. *Test 43*, light discrimination time: correlates with thirst tests (exclusive of drinking tests), moderately with the hunger tests (exclusive of eating tests), with the sex tests, and negatively with emotional defecation.

(f) *Intercorrelations of emotional defecation, body weight and activity; and their trends to correlation with other groups of tests.* Table 42 presents the intercorrelations between these three tests. This table completes the presentation of the intercorrelations of all of the 47 tests given. Additional tables to be presented in following sections do not present new correlations but merely a regrouping of correlations already presented in tables 22 to 42. Of the three correlations of table 42, only the correlation of −.41 between emotional defecation, test 4, and activity, test 47, is high enough to merit consideration. This correlation is 2.8 times its standard error and indicates that emotion may prevent adaptation to the activity drums and hence result in a small amount of activity, even though the rats of the age used are given one month to adapt to the drums before scores are taken.

(1) Summary of trends to correlation of the emotional defecation test 4 with all other tests. In general, a high defecation score is associated with a 'poor' performance on other tests. The correlations of the defecation test with exploratory and thirst tests are low but the defecation test shows a moderate tendency to correlate negatively with the hunger tests, and much stronger tendencies to negative correlation with the sex tests and with the learning measures. In the present experiment considerable effort was made to have the animals well adapted to any given piece of apparatus before the tests on that apparatus were given. Even under these favorable conditions, however, emotional factors appear to be important enough to result in moderate but consistent negative correlations between the emotional defecation test and other tests.

TABLE 42

Intercorrelations between the emotional defecation, body weight and activity tests

TEST NUMBER	NAME OF TEST	N	TESTS	
			46	47
4	Emotional defecation	51	−.244	−.408
46	Body weight	51		.226
47	Activity	34		

(2) Summary of trends to correlation of body weight, test 46, with all other tests. Body weight shows little tendency to correlate with any group of tests when such tests are classified according to drive. It gives relatively high correlations with drinking amount, test 14, with eating amount, test 20, and with vaginal plugs, test 37, and shows a negative correlation with the

vertical maze test 2. Body weight also correlates with all tests using the sand apparatus, the correlations ranging from .39 to .57.

(3) Summary of trends to correlation of activity, test 47, with all other tests. Because of the limited number of cases (N = 34) on this test, little confidence can be placed in trends involving correlations of the size obtained. There is some tendency for the activity test to correlate with sex tests and with learning measures, and also a tendency for the activity test to correlate with tests involving the speed runway.

IV. DISCUSSION

The more general aspects of the correlations presented in the preceding section will be discussed under six headings, namely, A. Intercorrelations between measures of the same drive; B. Intercorrelations between measures of different drives; C. Intercorrelations between measures of drives and measures of learning performance; D. Apparatus aptitudes; E. The influence of position in the testing program and of repeated experience with identical apparatus; and F. General interpretative discussion.

A. Intercorrelations between Measures of the Same Drive. In an earlier study by the writer (2), significant correlations were found between two different tests of exploration and also between two different sex tests, but there was little evidence of intercorrelation between different hunger tests when the possible effect of other factors was taken into consideration. The previous study was inconclusive because of the limited number of tests of any given drive. In the present study, however, a variety of tests was used for each drive, and the same general results were obtained; all tests of exploration tend to correlate with all other tests of exploration, and all sex tests correlate with all other sex tests, hunger tests do not show any general tendency to correlate with other hunger tests, nor do thirst tests, in general, correlate with other thirst tests. The results of the present study, therefore, show a clear differentiation between thirst and hunger tests on the one hand and exploratory and sex tests on the other, in so far as the tendency of different measures of the same drive to intercorrelate is concerned. Although thirst and hunger appear to be stronger drives than exploration and sex [c.f. Columbia studies (13) and page 46 of the present study], the latter two drives seem to be much more important than hunger and thirst in determining individual differences upon tests involving the given drive. There are probably a number of factors which may be influential in producing the different correlational results with exploration and sex as contrasted with hunger and thirst; seven possible factors will be considered.

1. *Possible influence of differences in the intensity of the drives.* Since one characteristic common to sex and exploratory drives is that they are both weaker than the thirst and hunger drives, it might be that the intercorrelations found in the group of exploratory tests and the intercorrelations within the group of sex tests are due to the fact that these drives are relatively weak. It would then follow that hunger or thirst tests given under a low degree of motivation should yield relatively high intercorrelations. Even if this were found to be true, the interpretation would not be easy, since hunger and thirst tested at a low degree of intensity might involve exploratory and emotional factors which would of themselves result in correlation quite independently of the intensity factor. However, if the correlational trends of the exploratory and sex tests are both due to the low intensity of these drives, it would follow that the correlations of the exploratory tests with the sex tests should be as high as the correlations within the group of exploratory tests or within the group of sex tests. A comparison of tables 22 (exploratory tests) and 25 (sex tests) with table 29 (exploratory vs. sex tests) shows that, while there are a few significant correlations between exploratory tests and sex tests, table 29 by no means yields the strong correlational trends present in tables 22 and 25. It therefore appears improbable that the correlations of exploratory tests with exploratory tests and of sex tests with sex tests can be attributed to the relatively low intensity of these two drives.

2. *Possible influence of testing a drive at different times.* In testing the hunger and thirst drives, the animals were placed upon a restricted food or water diet for several days before the

tests were given, and since neither the hunger tests nor the thirst tests were all given consecutively, it might appear that the animals might be differently motivated at different times during the testing schedule. However, the exploratory and sex tests were also given at various times in the testing schedule, and hence this factor could hardly account for the different results obtained with these tests as contrasted with hunger and thirst tests. Furthermore, the fact that the sand, speed, and obstruction tests with hunger motivation, for example, show low intercorrelations although they were given consecutively during the same restricted food period indicates that the low intercorrelations between the hunger tests were probably not due to the fact that these tests were given at different times and under different restricted diet controls.

3. *Possible influence of position in the testing program.* It will be pointed out later that tests occurring late in the testing program tend to show higher correlations than tests occurring early in the program. And since, in all cases of different drives being tested on the same apparatus, the order was uniformly hunger, thirst and sex, it might appear probable that the low intercorrelations within the group of hunger tests and within the group of thirst tests should be attributed to this artifact of testing order. But the fact that the exploratory tests given early in the program show high correlations with the exploratory test given late in the program, and the fact that the direct sex test given early in the program correlates significantly with the sex tests occurring late in the program indicates that exploratory tests correlate with exploratory tests and that sex tests correlate with sex tests regardless of the position of these tests in the testing program.

4. *Possible influence of unique tests of different drives.* Although a number of hunger and thirst tests were very similar to some of the exploratory and sex tests, it was not possible to duplicate all tests for each of the drives. The direct tests, of course, are unique for each drive. The sand digging test, the obstruction test, and the speed of running test were employed with hunger, thirst and sex motivation. Certain other tests were common to

at least two of the different motivations. In view of the communality of so many of the tests it would seem untenable to attribute the difference in the intra-drive correlations obtained with sex and exploration as contrasted with the hunger and thirst relations, to the uniqueness of certain tests used for the different drives. The factor of unique tests would appear to be most important in the case of the exploratory drive since six of the seven tests of this drive were of the direct type and hence unique. However, the fact that the one indirect exploratory test (the obstruction exploratory test) correlates significantly with four of the six direct tests indicates that the type of test used was not the only factor producing correlation among the different exploratory tests.

5. *Possible influence of differences in the reliabilities of the tests.* If the hunger and thirst tests were unreliable as compared to the sex and exploratory tests, the low intra-drive correlations of the first two drives might be attributed to this factor. However, the obtained reliability coefficients clearly indicate that such an interpretation is untenable. The average reliability coefficient for the exploratory tests is .87, for the sex tests .85, for the hunger tests .86 and for the thirst tests .90.

6. *Possible influence of some factor more general than the drive itself.* The correlations of exploratory tests with other exploratory tests, and likewise for sex tests, may be due to some factor more general than the drive itself. Because of the nature of the exploratory tests, it might appear reasonable to suppose that 'spontaneous activity' may be at least one such factor; but the failure of the exploratory tests to show any tendency to correlate with activity as measured in the revolving drums is opposed to such an interpretation. In the case of the sex tests, the tendency of the sex tests to correlate with the learning measures, with the emotional defecation test, with the activity measure, and sporadically with some other tests, indicates that the correlations of sex tests with other sex tests may in part, at least, be due to a factor or factors broader than the specific sex drive. It would seem, however, that such factors would be inadequate to account for all the correlations of sex tests with sex tests, and that the corre-

lations can be attributed in part, at least, to the common sex motivation.

7. *Possible influence of the mode of arousal of the drives.* While the factors accounting for correlations between the different sex tests may not be the same as those accounting for the correlations between the different tests of exploration, there is one similarity between measures of sexual behavior and measures of exploratory behavior which sets them off from measures of hunger or thirst behavior. This similarity lies in the mode of arousal of the drives. Some drives appear to be primarily aroused by internal changes occurring within the organism while other drives seem to be primarily aroused by the external situation. The distinction is not absolute and it is probable that both internal and external stimuli are influential in the arousal of any drive. Hunger and thirst appear to be aroused primarily by internal factors; in order to get an animal to work at all it is necessary to deprive him of food or water and thus produce internal changes in the organism. On the other hand, the exploratory drive seems to be primarily aroused by the external situation. No attempt is made to alter the animal's internal condition; the rat is placed in an appropriate environment and the external situation arouses the exploratory behavior. The sex drive in the *male* rat also appears to be largely dependent upon external stimulation for its arousal although internal factors may be more important than in the case of the exploratory drive. No attempt is made to alter the internal conditions of the organism; the external situation, i.e. the female in oestrum, serves to incite the male to sexual activity. The external situation or stimulating conditions thus appear to be of more or less primary importance in the arousal of exploratory or sexual behavior, while the internal conditions of the organism are of primary importance in arousing hunger and thirst behavior. In a later section it will be argued that the differences in the correlations obtained with the different drives may possibly be accounted for in terms of this difference in mode of arousal.

Summary. Sex tests correlate with other sex tests and exploratory tests correlate with other exploratory tests. No such intra-drive relationship is evident within the group of thirst tests nor within the group of hunger tests. The differences in correlational trends may be due to the nature of the drives themselves, especially with reference to the mode of arousal, or may be due to the operation of a factor or factors more general than the specific drive.

B. Intercorrelations between Measures of Different Drives. Tables 27 to 42, previously presented, indicated that, in general, there was very little tendency for all of the tests of any one drive to correlate with all of the tests of another drive. The group of sex tests did tend to correlate with the group of learning measures, but in other cases high correlations between tests of different groups appeared to be sporadic. In the presentation of the data of tables 27 to 42, it was noted that there were a fair number of cases of individual tests tending to correlate with most of the tests of a given group. The emotional defecation test, for example, showed a moderate tendency to correlate negatively with the group of hunger tests, some tendency to correlate negatively with the learning measures, and a strong tendency to negative correlation with the sex measures. Again, some hunger tests, for example, showed more marked tendencies to correlate with the group of thirst tests than they did with the other hunger tests, and might even correlate better with the thirst tests as a group than did the other thirst tests. In the case of some of the exploratory tests, a given exploratory test might correlate well with the group of hunger tests or some other group of tests but it would also correlate with other exploratory tests. The data obtained thus indicate that the performance on any given test may be a complex function of a number of factors, many of which are probably quite independent of the particular drive involved.[7] As was pointed out in the preceding section, the common drive does, in some cases, appear to be important in determining correlations, but in other cases the specific drive or drives involved appear to be of little or no importance.

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[7] It is possible that methods of factor analysis may aid in the clarification and identification of the factors involved. At the present time the writer feels that the labor necessary to carry out an adequate analysis of the data of the present study would not be justified by the probable outcome of the analysis, especially in view of the fact that the correlations of this study are based upon the relatively small population of 51 cases.

While it does not appear to be probable that any very broad factor which could be identified as a 'general drive' factor will be found, yet an examination of the intercorrelations obtained between some of the direct tests of the different drives indicates that possibly some factor common to several of the direct tests may exist. Intercorrelations among some of the direct tests of different drives (selected on the basis of the size of the correlations) are presented in table 43. The drinking tests 15 and 16 yield moderate correlations with the direct tests of other drives but the signs are consistently negative, except with the defecation test. The negative correlations mean that rats tending to drink most tend to copulate less frequently, for example. The direct hunger tests 20 and 21 yield only moderate trends to correlation with direct tests of other drives, but several of the correlations with the sex tests are significant. The copulation tests 5, 36, and 37 show the most marked trend to correlation with the direct tests of other drives. Twelve of the eighteen correlations involved are .35 or over. The defecation test 4 correlates negatively with most of the other tests. The negative correlations mean that animals defecating least tend to make 'good' per-

TABLE 43

Intercorrelations between direct tests of different drives

TEST NUMBER	NAME OF TEST	TESTS						
		20	21	5	36	37	4	3
15	Drinking amt. per gm. wt.	−.32	−.26	−.38	−.25	−.35	.20	−.09
16	Drinking time	−.18	−.28	−.48	−.25	−.29	.26	−.26
20	Eating amount			.44	.37	.48	−.28	.14
21	Eating amt. per gm. body wt.			.45	.31	.40	−.24	.17
5	Copulation						−.44	.32
36	Copulation retest						−.40	.35
37	Vaginal plugs						−.50	.17
4	Emotional defecation							−.33
3	Open field exploration							

formances upon the other tests. The exploratory test 3 shows the least tendency to correlate with the direct tests of the other drives. The data presented in table 43 may thus indicate the existence of some factor or factors common to several of the different drives as measured by direct tests. It should be noted, however, that some of the correlations are not significant, and that not all direct tests yield correlations as high as those of table 43. It should also be emphasized that if the copulation tests are omitted from consideration, the rest of the table offers little evidence of inter-drive relation. Hence the argument for a 'general drive' factor rests largely upon the tendency of the copulation tests to correlate with direct tests of other drives. With reference to Freud's conception of the *libido* it is perhaps of interest to note that the tests of sexual behavior yield more significant correlations with all of the other tests than do the tests of any other drive.

C. Intercorrelations between Measures of Learning and Measures of Drives. In an earlier study (2), the writer found a fairly widespread tendency for maze scores to correlate with motivational measures. The maze test of the earlier study was given under a relatively low intensity of motivation and without adaptation. The motivational measures were also given with what the writer now believes to be completely inadequate adaptation. Under the conditions of that study, then, it appears very probable that exploratory and emotional factors were important determiners of a number of performances, and hence relatively high intercorrelations could be expected. With the more adequate adaptation and improved techniques of the present study, however, the learning measures show very little tendency to correlate with the motivational tests. In general, the learning measures correlate positively with the sex tests and negatively with the emotional defecation test, and show a very moderate and inconclusive trend to correlation with the activity measures. The maze error scores also correlate negatively with the obstruction exploratory test and there are a few sporadic correlations with other tests, but the widespread tendency to correlation found in the earlier study is not evident. It is of some interest to note that the hunger motivation used with the learning tests does not result in correlations with hunger tests. The present study then, indicates that there is some relationship between learning performance and some tests of motivation, but that the correlations are largely restricted to tests of the sex drive, emotional defecation and possibly exploration and general activity.

D. Apparatus Aptitudes. Throughout the presentation of the data of tables 22 to 42 some correlations were attributed to a factor associated with the identity of the apparatus involved in giving the two tests. Such a factor may be termed an aptitude or ability for the particular type of performance required in the tests using a given apparatus. Evidence for such aptitudes is available in cases in which the same apparatus was used with two or more different motivating conditions and will be considered according to the apparatus involved.

TABLE 44

Intercorrelations between tests given on the sand digging apparatus

TEST	MOTIVATION	SAND APPARATUS, TOTAL TIME		WITH BODY WEIGHT PARTIALLED OUT	
		Tests			
		32	38	32	38
26	Hunger	.794	.681	.728	.591
32	Thirst		.810		.750
38	Sex				

1. *Jump apparatus aptitude.* Only two tests, the jump hunger test 12 and the jump thirst test 19, were given with the jump apparatus. The correlation between these two tests is .57. This correlation is considerably higher than any correlation of either of the two tests with any other test, and therefore is to be attributed, in part at least, to an aptitude for the jump tests.

2. *Sand apparatus aptitude.* The intercorrelations between the three total time scores of the tests given on the sand apparatus are presented in table 44. Since these tests all show substantial correlations with body weight, their intercorrelations with body weight partialled out by Yule's formula are also pre-

sented in the same table. The partial correlations are but slightly lower than the original correlations, and it is therefore probable that the correlations are to be attributed to a sand test aptitude.

3. *Speed runway aptitude.* The intercorrelation of the three speed tests based upon the two fastest trials of five are presented in table 45. The relatively high intercorrelations indicate the existence of a speed runway aptitude, but is possible that the intercorrelations are at least partially due to the operation of some more general factor such as "spontaneous activity" since the speed tests yield correlations of .26, .42, and .55 with the activity test 47. The relatively low correlation of .27 between the speed hunger test 27 and the speed sex test 39 may be partially due to the temporal separation of these two tests.

TABLE 45

Intercorrelations between tests given on the speed runway

TEST	MOTIVATION	SPEED RUNWAY, FAST TRIALS	
		Tests	
		33	39
27	Hunger	.565	.272
33	Thirst		.637
39	Sex		

4. *Obstruction apparatus aptitude.* The intercorrelations between the six tests given on the obstruction apparatus are presented in table 46. There is again evident a general tendency for tests given on the same apparatus to intercorrelate, but the correlations are on the whole considerably smaller than those presented in the immediately preceding paragraphs. The wide variation in the strength of the shock used (from no shock to a very strong shock) may be one factor tending to reduce the intercorrelations among the obstruction tests. In the earlier study (2) the obstruction tests showed somewhat higher intercorrelations that those of the present study, but the differences in technique in the two studies are more than adequate to account for the obtained differences in result. The obstruction apparatus aptitude appears to be the least important of the four

aptitudes considered, and, in general, the obstruction tests yield several correlations with non-obstruction tests which are as high, or higher, than those with the other obstruction tests.

E. The Influence of the Position of Tests in the Testing Program. At least two factors affecting the size of the correlations appear to be associated with position in the test schedule, namely: 1. Temporal proximity of the tests, and 2. A factor associated with increased experience upon the tests used.

1. *The effect of temporal proximity of tests upon the intercorrelations between those tests.* Other factors being equal, two tests occurring close together in the testing program may be expected to show a higher correlation than the same two tests would show if they were more widely separated in time. The effect of temporal proximity was most clearly seen in the correlations of the copulation test 5 and the copulation retest 36 with other sex tests. In the earlier discussion it was pointed out that, although a temporal separation of four months or more appeared to reduce the correlations between two sex tests by from .09 to .27 correlation points, significant correlations were still obtained between these tests. If two tests, widely separated in the testing program fail to yield significant correlations, it is in general impossible, on the basis of the present data, to determine whether that absence of correlation should be attributed to temporal changes in the organism or to the independence of the two tests. There-

TABLE 46

Intercorrelations between tests given on the obstruction apparatus

TEST NUMBER	MOTIVATION	OBSTRUCTION APPARATUS				
		Tests				
		29	34	35	40	41
13	Hunger no shock	.349	.319	.308	.324	.425
29	Hunger with shock		.291	.490	.296	.268
34	Thirst, strong shock			.633	.337	.227
35	Thirst				.497	.385
40	Sex					.388
41	Exploration					

fore the factor of temporal separation of tests should be considered in interpreting any given case of absence of correlation.

2. *The effect of increased experience in tests upon the correlations between those tests.* Two different tests which fail to correlate the first time they are given may correlate significantly when presented again later in the testing program. The increase in correlation at later presentation is most clearly seen in the case of the sand, speed, and obstruction tests with hunger, thirst, and sex motivation. Intercorrelations between these three tests for each of the three motivating conditions are presented in table 47. The three hunger tests, which were the first tests given with the sand, speed and obstruction apparatuses, show low intercorrelations ranging from .17 to .21. Later in the test schedule, the animals were tested on the same apparatuses but with thirst motivation. The intercorrelations between the tests with thirst motivation range from .27 to .54 and are thus much higher than the corresponding hunger test correlations. When the apparatuses were used a third time with sex motivation the correlations are still higher, ranging from .51 to .64. There is thus evident a progressive increase in the size of the correlations with repeated experience upon the same apparatus. However, since the motivation differed each time the apparatuses were used, the differences in the size of the correlations might possibly be attributed to the differences in motivation. The evidence presented earlier showed that sex tests tend to correlate with other sex tests regardless of position in the testing program. The

TABLE 47

Intercorrelations of the sand, speed, and obstruction tests under three conditions of motivation, showing the possible effect of repeated presentation of tests upon the intercorrelations between the tests

HUNGER MOTIVATION TESTED BETWEEN THE AGES OF 236 AND 250 DAYS				THIRST MOTIVATION TESTED BETWEEN THE AGES OF 270 AND 285 DAYS				SEX MOTIVATION TESTED BETWEEN THE AGES OF 289 AND 311 DAYS			
Test number		Tests		Test number		Tests		Test number		Tests	
		27	29			33	35			39	40
26	Sand hunger	.171	.210	32	Sand thirst	.543	.269	38	Sand sex	.639	.512
27	Speed hunger		.187	33	Speed thirst		.414	39	Speed sex		.523
29	Obstruc. hunger			35	Obstruc. thirst			40	Obstruc. sex		

argument of the present section is that, while the correlations between the sex tests 38, 39 and 40 are in part due to the common sex motivation, they may also be partly due to the fact that the apparatuses involved had been previously used.

The best situation to test whether or not repeated testing tends to increase the size of the correlations obtained would be one in which the apparatuses were used repeatedly with the same motivation. In the absence of some such crucial test, other evidence as to the progressive increase in size of correlations with increased experience on the animal's part is presented in table 48. This table presents the correlations of the sand tests given under hunger, thirst and sex motivation with the speed tests and with the obstruction tests given under the three different conditions of motivation. It will be noted that the sand hunger test 26 correlates .17 with the speed hunger test (the first speed test), .34 with the speed thirst test (the second speed test), and .36 with the speed sex test (the third speed test). It is thus evident that the sand hunger test yields higher correlations with the speed tests given relatively late in the program than it does with the first speed test given, even though the first speed test was given with hunger motivation and was the next test given after the sand hunger test. This tendency to progressive increase in the size of the correlations with tests occurring later in the testing program is also found in other similar comparisons in table 48.

TABLE 48

Intercorrelations of the sand tests with the speed and obstruction tests given under three different conditions of motivation

TEST NUMBER		SPEED TESTS			OBSTRUCTION TESTS		
		Motivation					
		Hunger	Thirst	Sex	Hunger	Thirst	Sex
		Tests					
		27	33	39	29	35	40
26	Sand hunger	.17	.34	.36	.21	.30	.38
32	Sand thirst	.23	.54	.59	.10	.27	.52
38	Sand sex	.20	.47	.64	.07	.12	.51

In view of the preceding discussion, it appears probable that repeated testing on similar tests tends to increase the correlations between the tests involved. It might be possible, however, that the tendency is due, not so much to the factor of increased experience upon the tests as to some extraneous factor associated with position in the test schedule. First, it might be due to age differences in the animals since one might expect younger rats to be less stable and thus to show lower correlations than they would when older. This interpretation is ruled out, however, by the conditions of the experiment, since the rats were adults at the time of the tests under consideration and since the difference in age was relatively slight at the time of the tests involved. The age of all animals at the start of the sand hunger test was 236 days, at the start of the sand thirst test, 270 days and at the start of the sand sex test, 289 days. Secondly, it might be possible that the differences in correlations were not due to the increased experience but were due rather to the influence of the various restricted diet periods on the rat's general physical condition. Since the animals had been subjected to various deprivation periods throughout the testing program, it is possible that they attained a more or less stable physiological condition and that the late tests thus became tests of general physical condition rather than tests of more or less specific performances. While this interpretation cannot be entirely disregarded, it appears to be improbable in view of the fact that all restricted diet periods were followed by rest periods and that the body weight of the individual rats remained quite constant with reference to the rats' relative position in the group.

The higher correlations obtained when tests are repeated, then, are probably most adequately accounted for in terms of the increased experience of the animals upon the tests. There is some indication that this tendency to yield higher correlations may not be entirely restricted to the specific test repeated, that is, that experience upon many varied tests may result in somewhat higher correlations between later but different tests, but the evidence is based largely upon the tendency of some of the later tests to yield higher correlations than earlier tests. At present, it is not possible to determine the nature of the generalized factor which appears to be produced by repeated testing. It may be motivational in character and due to the generalization of the more or less specific drives, but it may equally well be cognitive in character and due to the integration of aptitudes or skills which were more or less specific in an earlier stage of the animal's development.

F. General Interpretative Discussion. The results of the present investigation indicate that one is not justified in assuming that *any* given drive is necessarily a unitary factor operating to produce significant and consistent intercorrelations among different tests of the specified drive. Whether or not such intra-drive correlations are obtained appears to depend to a marked degree upon the particular drives being tested. The exploratory and sexual tests yield consistent and significant intra-drive correlations whereas the hunger and thirst tests do not. The question at once arises as to what factors may be responsible for such differences. In an earlier discussion several factors, including some artifacts of the testing procedures, were considered inadequate to account for the obtained differences in intra-drive correlations. It was further suggested that the differences might be related to the mode of arousal of the various drives. An attempt to account for the obtained differences in intra-drive correlations. upon the basis of the mode of arousal of the drives will be made in the present section. Because of limited knowledge concerning the mechanisms involved in drives it is impossible to present a completely adequate theory; the following discussion should therefore be considered as extremely tentative.

In the earlier discussion (p. 97) the hunger and thirst drives were regarded as being primarily aroused by the internal conditions of the organism while the sex and exploratory drives were considered as being more dependent upon the external situation for their arousal. Assuming this classification to be correct, there appear to be at least two factors associated with mode of arousal which would tend to result in lower intra-drive correlations in the case of an internally aroused drive than in the case of one externally aroused. In the first place, it is probably easier to obtain *consistent* arousal of a drive which is dependent primarily upon the external situation than of a drive which is more dependent upon the internal conditions of the organism. An externally aroused drive is incited by presenting some particular aspect of the external situation; the sex drive, for example, is aroused by presenting a female in oestrum. By careful selection it is possible to obtain females which are very similar with respect to the degree of receptivity, and thus the essential external conditions which arouse the drive can be duplicated quite closely. With the internally aroused drives, however, it is necessary to alter the internal state of the organism in order to arouse the drive. The essential internal conditions for the arousal of such drives are unknown, nor is it known how such conditions could be produced directly. The internally aroused drives, therefore, are excited somewhat indirectly by controlling the food or water diet. Because of the necessity of this indirect arousal, it is probable that the inciting conditions of the internally aroused drives can not be duplicated from time to time as closely as they can with the externally aroused drives. This factor, then, should tend to lower the intercorrelations between different measures of an internally aroused drive.

Secondly, there is a somewhat greater probability that disturbing factors may be present in testing an internally aroused drive than in testing an externally aroused drive. Since any drive must be tested in an external situation, there is always a possibility that the situation may arouse a drive or drives (such as exploration or emotion) which may conflict with the drive being tested, and hence lower intercorrelations may result. If, for example, there are n drives which might be aroused by a given situation, an internally aroused drive such as hunger or thirst might conflict with any or all of these n drives. In testing an externally aroused drive such as sex or exploration, however, the drive being tested is one of those aroused by the external situation and thus can conflict with a maximum of $n - 1$ extraneous drives aroused by the environment. The probability is thus somewhat greater that disturbing factors may occur in testing an internally aroused drive than in testing one which is externally aroused. The two factors just considered would thus both operate to produce lower intra-drive correlations with the hunger and thirst drives (internally aroused drives) than with sex and exploration (externally aroused drives), and the obtained differences in intra-drive correlations may thus be related to the mode of arousal of the drives.

There are several facts which appear to indicate that the hunger and thirst drives can become externally aroused. Rats that have thoroughly learned a given form of behavior when motivated by the hunger drive may persist in this behavior for a time even though they are satiated before being placed in the apparatus. For example, a rat may persist in depressing a lever even though he may not eat the food thus obtained; a rat thoroughly trained upon a maze may make very few or no errors for a trial or two after satiation. Such facts as these would thus seem to indicate that behavior associated with the hunger drive may be aroused by the external situation in the absence of the internal conditions here assumed to be requisite for the arousal of the hunger drive. These facts, however, are not necessarily contradictory to the present view that the hunger and thirst drives are internally aroused, but do necessitate a modification. That is, a drive which was originally internally aroused may become externalized. Through a process of learning or conditioning a given situation may come to arouse, at least to some degree, a drive (or response ?) which was originally dependent upon internal conditions. The above cited cases of the persistence of a given response in a satiated animal might be interpreted as instances of a habit becoming a drive or as instances of a new motivation maintaining an old response, but would here be considered as instances of an internally aroused drive becoming externalized.

If, then, such internally aroused drives as hunger and thirst can become externalized, the question may legitimately be raised as to why such externalization failed to take place in the present experiment sufficiently to result in correlations between the different hunger tests or between the various thirst tests. The answer is that such externalization probably did take place, but that for the most part it was restricted to one single piece of apparatus, and thus may be expressed in the reliability coefficient of a given test. If the preceding discussion is correct in assuming that it is difficult to obtain consistent arousal of the hunger and thirst drives, one might expect that such internally aroused drives would yield lower reliability coefficients than the externally aroused drives. It has been pointed out, however, that the hunger and thirst tests are not less reliable than the sex and exploratory tests. While the reliability coefficients may in part be due to the operation of an aptitude for the particular test involved, it is also possible that they are partly influenced by the externalization of the drive in that particular situation. It would presumably require a greater degree of externalization in order to obtain correlations between two tests given in different situations than to obtain correlations (i.e. reliability coefficients) between tests given on the same apparatus, and it would seem that this greater degree of externalization was not obtained under the conditions of this experiment. In an earlier discussion it was pointed out that some thirst tests occurring relatively late in the testing program showed higher intercorrelations than did the same tests given earlier in the program with hunger motivation. This effect was there attributed to the increased experience of the animals upon various apparatus and it was suggested that the effect might be due to some process of generalization of a drive or other function which appeared to be produced by the repeated testing upon different apparatuses. The present discussion indicates that another interpretation is possible; that is, the higher correlations obtained later in the program are still to be attributed to the increased experience of the animals upon varying apparatuses, but the process of generalization is further specified to be one of the externalization of drives which were originally more dependent upon the internal conditions. Had the animals been given further hunger or thirst tests upon the same pieces of apparatus so that these drives had become externalized to a greater degree, then they might ultimately have shown intra-drive correlations as high as those obtained with the sex and exploratory drives. To summarize: The preceding discussion has attempted to justify two assumptions, namely (1) that various tests of externally aroused drives should yield higher intercorrelations than tests of internally aroused drives, so long as the latter drives remain primarily internally aroused; and (2) internally aroused drives tend to become externally aroused through experience. These two assumptions are then used to explain the presence of intra-drive correlations with the sex and exploratory drives and the absence of such correlation with the hunger and thirst drives, and to explain the tendency of tests occurring late in the program to yield higher correlations than do similar tests occurring earlier in the program.

Certain implications would appear to follow from the above assumptions. (a) Different tests of an internally aroused drive, if repeated frequently enough, and over a sufficiently long period of time should, because of the externalization of the drive, ultimately yield higher intercorrelations than when first tested. The size of the final correlations obtained would presumably depend upon the extent to which the given drive can become externalized in the given animal species. (b) With reference to the sex drive in the *female* rat, results somewhat at variance with those obtained with the male rat may be expected. In the female, the sex drive appears to be primarily internally aroused, and thus various tests of the sex drive should yield lower intercorrelations than those obtained with the male rat. The experimental situation may be difficult to handle since some contact with a male rat would presumably be necessary as a reward and it might not be easy to prevent a rapid externalization of the drive. (c) The maternal drive presents an interesting case. This drive is apparently at least partially dependent upon internal conditions, but at the same time experiments upon concaveation indicate that it may be aroused through external conditions. If it were possible to determine what aspects of maternal behavior could be aroused by the external situation (i.e. by concaveation) and what aspects could be aroused only by producing internal changes in the organism (either by normal pregnancy or glandular injections), some prediction as to correlations between the various tests might be made. (d) Experiments upon the social and other facilitation of eating and upon appetites or food preferences in animals would appear to indicate that some aspects of the hunger drive may be externally aroused without extensive preliminary training. Hence, if a food could be found which was so preferred that the animals would work for it even though not hungry, or but slightly so, then various tests given with this food as reward should intercorrelate. (e) A final implication of the hypothesis presented may be stated with reference to emotional behavior. Since emotional responses of the rat are primarily aroused by the external environment, different measures of emotional behavior should yield significant intercorrelations.

In the preceding discussion an attempt was made to justify two assumptions. These two assumptions were then used to explain some of the results of the present study. Several implications which are subject to experimental verification were then pointed out.

V. SUMMARY

1. Intercorrelations were obtained among 47 tests given to a group of 51 male albino rats. There were 7 exploratory tests, 10 thirst tests, 14 hunger tests, 7 sex tests, 6 learning scores, and one measure each of emotion, general activity, and body weight.

2. Most of the tests yielded satisfactory reliability coefficients; 40 of the reliability measures lying within the range of .80 to .99.

3. Intercorrelations between different measures of the same drive:

A. In general, exploratory tests correlate significantly with other exploratory tests.

B. In general, thirst tests do not correlate significantly with other thirst tests.

C. In general, hunger tests do not correlate significantly with other hunger tests.

D. In general, sex tests correlate significantly with other sex tests.

4. The exploratory and sex drives appear to be important in determining individual differences upon tests involving those drives, whereas the hunger and thirst drives appear to be relatively unimportant in the determination of individual differences in performance upon hunger and thirst tests. It is suggested

that these differences may be related to the fact that hunger and thirst are primarily internally aroused drives, whereas sex and exploration, in the male rat, are primarily externally aroused.

5. Learning measures tend to correlate with other learning measures, the time scores yielding higher correlations than do the error scores.

6. Intercorrelations between measures of different drives, on the whole, are somewhat sporadic in occurrence, and there is little evidence of any important 'general drive' factor influencing performance upon a large number of tests. There is, however, some slight indication of relationship between such direct tests of different drives as eating, drinking, and copulation tests.

7. Some of the tests appear to be complex in nature and may correlate with tests of several other drive groups. Some hunger tests, for example, yield higher correlations with thirst tests than they do with other hunger tests.

8. There is a fairly general tendency for sex tests, learning measures and the emotional defecation test to yield significant or almost significant intercorrelations. There is thus some evidence of the operation of a general factor or factors within these groups of tests.

9. The emotional defecation test tends to correlate negatively with a large number of tests, but the tendency is most marked with sex tests and with learning measures. There is an indication that the correlations of the defecation tests with learning measures tend to decrease as different learning tests are given.

10. Learning measures, on the whole, show little tendency to correlate with motivational measures with the exception of those tests previously mentioned.

11. There is some evidence that repeated and varied testing may result in higher correlations between tests. Two different tests which fail to correlate the first time they are given may correlate significantly when presented again later in the testing program. This effect may be due to the generalizing of a drive factor, to the integration of aptitudes which were more or less specific in an earlier stage of the animal's development, or to the externalization of drives which were originally internally aroused.

12. Tests given on the same apparatus, but with different motivation, show, in general, relatively high intercorrelations, thus indicating the operation of 'apparatus aptitudes.'

13. Significant intercorrelations may be obtained between sex tests separated by four months, but such a temporal separation appears to lower the correlations from .09 to .27 correlation points.

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VII. APPENDIX. COMPARISON OF THE RESULTS OF THE PRESENT STUDY WITH THOSE OF THE EARLIER STUDY (2)

To simplify comparison between the two studies, the correlations between tests which were common to the two studies are presented in tables 49A and 49B. It should be recalled, however, that in no case are any tests of the two studies exactly similar. There were marked differences in techniques and in the preliminary adaptation of the animals to the apparatus used, so that an obstruction test of the present study, for example, is by no means an exact duplicate of an obstruction test of the earlier study. In general, the correlations obtained in the earlier study are higher than those of the present study. Since the tests of the earlier study were given with somewhat inadequate preliminary adaptation, and with a lower intensity of motivation, it is probable that the test situations may have aroused emotional, exploratory, and general activity factors which may have influenced the scores on a number of tests. The influence of such factors on a number of tests might not only increase the size of the correlations between tests, but might even produce significant correlations between tests which would not otherwise correlate. While differences in technique and mode of testing other than those just mentioned may also have influenced the correlations obtained, the writer believes that most of the cases of discrepancy between the results of the two studies can be adequately accounted for in terms of the factors just considered. The data of tables 49A and 49B will be compared in detail according to the particular tests involved.

A. Correlations of Obstruction Tests with other Obstruction Tests. The data of the two studies are in essential agreement as to the tendency of obstruction tests to correlate with other obstruction tests. The correlations of the present study are somewhat lower than those of the earlier study, the average correlation for the group of tests under consideration being .370 for the present study and .459 for the earlier study. This difference in the size of the correlations is probably to be attributed to the factors mentioned above, but may be partially due to the fact that the obstruction tests of the present study were given with varying intensities of shock. In any case, the difference is not great.

B. Correlations of Activity with other Tests. In the earlier study the activity tests showed a fairly general tendency to correlate with other tests, but in the present study the correlations are low, although, as in the previous study, there is some tendency for the activity measure to correlate with learning measures and the copulation tests. The higher correlations of the earlier

TABLE 49

Comparison of the correlations of the present study with those of the earlier study (2)

The tables present the correlations between tests which are common to the two studies. In no case, however, were the tests of the two studies given under identical conditions.

A. Intercorrelations between tests of the present study

Blank spaces in the table indicate that no comparable correlation was obtained in the earlier study.

TEST NUMBER	NAME OF TEST	TESTS															
		29	34	35	40	41	47	30	31	44	45	42	43	5	36	1	46
13	Obstruction no shock hunger	.35	.32	.31	.32	.43	.24	.10	.25	−.23	−.03	.17	.20	.32	.43	.29	−.08
29	Obstruction hunger with shock		.29	.49	.30	.27	−.16	−.07	.00	.02	.20	−.12	.26	.19	.14	.03	.02
34	Obstruction thirst strong shock			.63	.34	.23	.05	.08	.14	−.11	.16	.28	.29	.06	.13	.12	−.25
35	Obstruction thirst				.50	.39	−.04	−.03	.14	.02	.20	.27	.37	.14	.14	.05	.00
40	Obstruction sex					.39	.24	.07	.33	.17	.35	.34	.52	.44	.64	.28	.02
41	Obstruction exploration						.03	−.37	−.05	−.35	−.11	−.05	.05	.16	.20	.52	−.07
47	Activity							.25	.25	−.03	.28	.29	.33	.32	.36	−.02	.23
30	U-maze A errors								.76					.16	.29	−.04	.07
31	U-maze A time													.12	.33	.21	−.15
44	U-maze B errors										.62			.35	.39	−.17	.25
45	U-maze B time													.25	.47	.00	−.02
42	Light discrimination errors												.77	.35	.42	.05	.06
43	Light discrimination time													.38	.56	.08	−.02
5	Copulation															.20	.17
36	Copulation retest															.19	.28
1	Dashiell maze exploration																−.33
46	Body weight																

B. Intercorrelations between tests of the earlier study (2)

The correlations are reprinted from Table 10 of the earlier study

TEST NUMBER	NAME OF TEST	TESTS											
		1R	2	2R	3	4	5	6	10	11	14	13C	16
1	Obstruction no shock hunger	.62	.27	.36	.40	.37	.45	.47	.37	.45	.32	.18	.11
1R	Obstruction no shock hunger retest		.18	.28	.43	.42	.35	.22	.49	.55	.39	.16	.05
2	Obstruction hunger with shock			.58	.54	.46	.32	.37	.21	.34	.10	.12	−.08
2R	Obstruction hunger retest				.56	.62	.55	.30	.23	.37	.17	.29	.08
3	Obstruction thirst					.70	.62	.43	.48	.58	.30	.33	.12
4	Obstruction sex						.56	.42	.51	.53	.51	.23	.22
5	Obstruction exploration							.39	.29	.44	.15	.63	−.14
6	General activity								.48	.57	.34	.12	.18
10	Multiple T-maze errors									.90	.77	−.10	.29
11	Multiple T-maze time										.66	.11	.33
14	Copulation											−.13	.37
13C	Dashiell maze corrected score												−.16
16	Body weight												

study may be partly attributed to the differences in technique mentioned above, but the fact that the activity test of the present study was given when the animals were over one year of age, and that there were numerous deaths and illnesses during the activity test, makes the results of the activity measure of the present study inadequate and inconclusive.

C. Correlations of the Learning Measures with other Tests. The maze error and time scores of the earlier study yield relatively high correlations with many of the other tests, but the data of the present study show no such tendency. A further disagreement between the two studies is evident in the negative correlations of −.37 and −.35 between the obstruction exploratory test and the maze error scores of the present study as contrasted with the positive correlation of .29 between the obstruction exploratory test and maze error score of the earlier study. In both studies, there is evidence of a relationship between learning measures and copulation scores, but the correlations of the previous study are considerably higher.

D. Correlations of the Copulation Tests with other Tests. The two copulation tests of the present study and the one copulation test of the previous study yield correlations which are essentially in agreement. In both studies, there is evidence of a relation between the copulation tests and the obstruction no shock hunger test, the obstruction sex test, and the learning measures, although the correlations in the latter case are considerably higher in the earlier study. There is also some inconclusive indication of correlation between the copulation and activity tests. In the earlier study, the copulation test correlated .37 with body weight, while the corresponding correlations of the present study are .17 and .28. However, the correlation of .41 between the vaginal plug score and body weight of the present study indicates that there may be some relation between body weight and sex performance.

E. Correlations of the Dashiell Maze Exploratory Test with other Tests. In the earlier study the Dashiell maze test correlated .63 with the obstruction exploratory test, but failed to yield any other significant correlations with the tests in table 49B. The results of the present study verify the earlier findings: the Dashiell maze test correlates .52 with the obstruction exploratory test, but fails to correlate significantly with any other test of table 49A.

F. Correlations of Body Weight with other Tests. Both studies are in essential agreement in showing little indication of relationship between body weight and the other tests.

Summary. In general, the correlations of the earlier study are higher than those of the present study, and some tests (maze and activity measures, e.g.) yielding significant correlations with other tests in the earlier study show no indication of such relationship in the present study. On the other hand, there are many cases of essential agreement. The discrepancies between the two studies can probably best be accounted for in terms of the inadequate preliminary adaptation and lower degree of motivation for the tests of the earlier study.

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